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SCIENCE

16 September 1960

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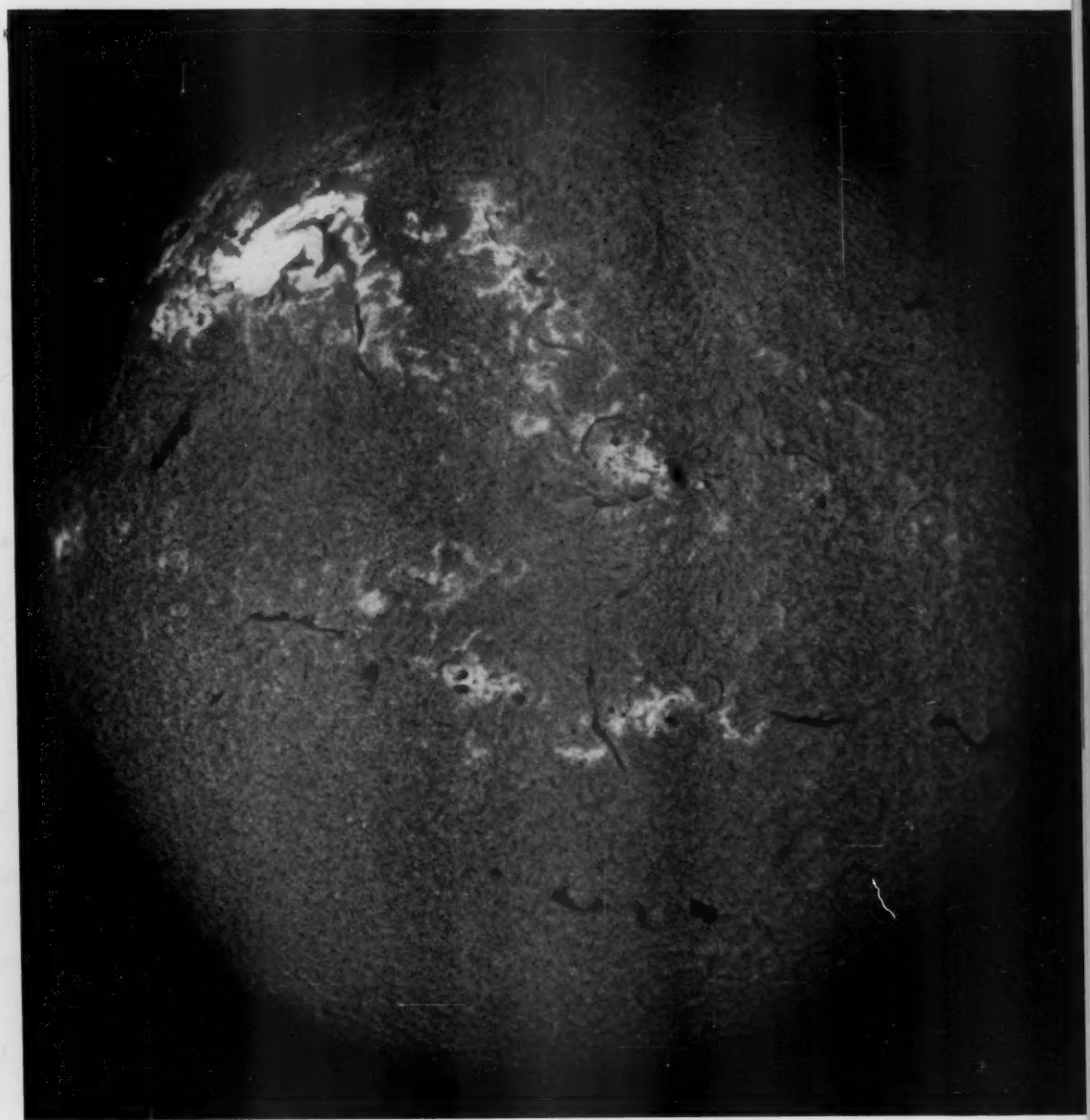

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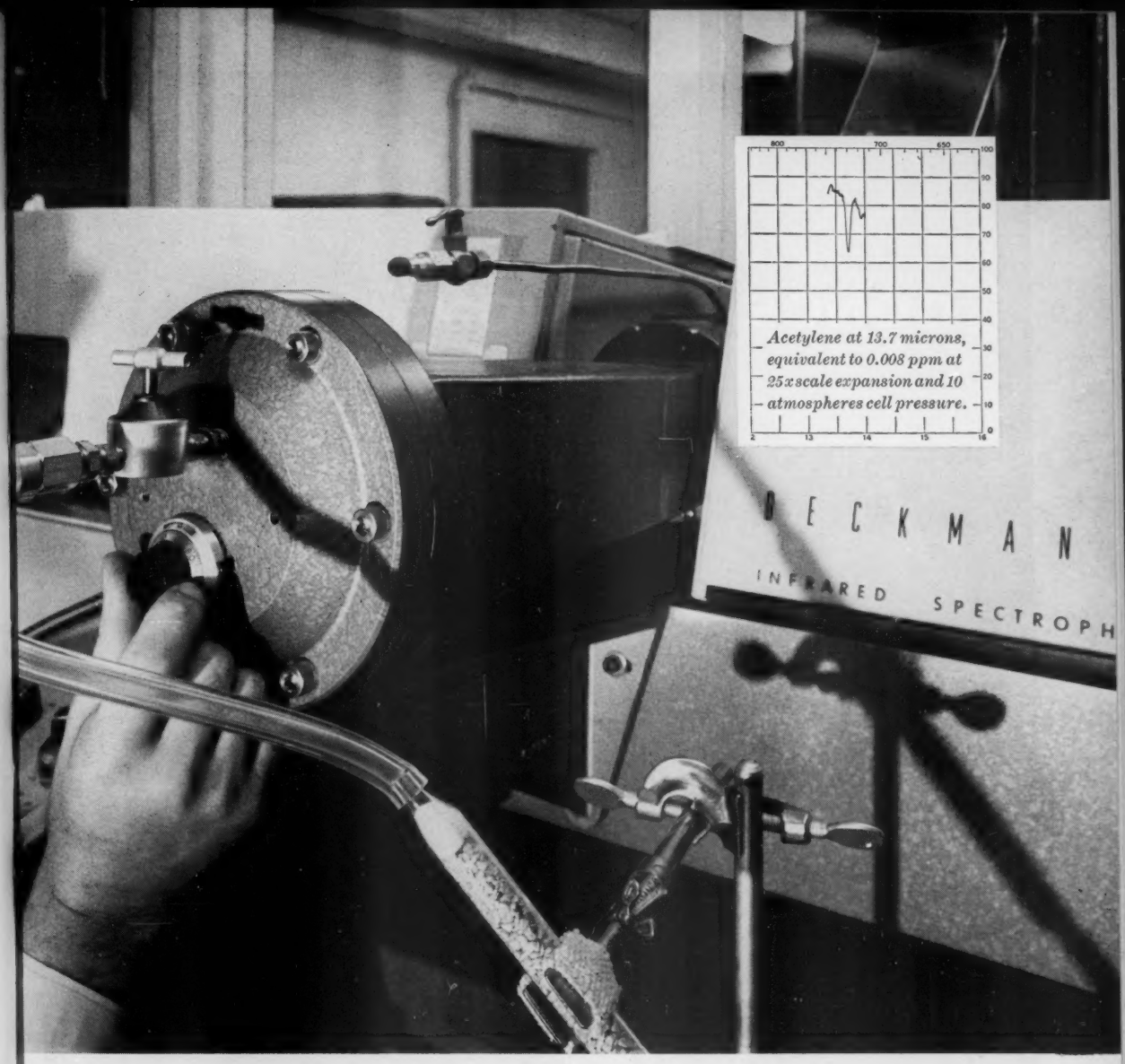

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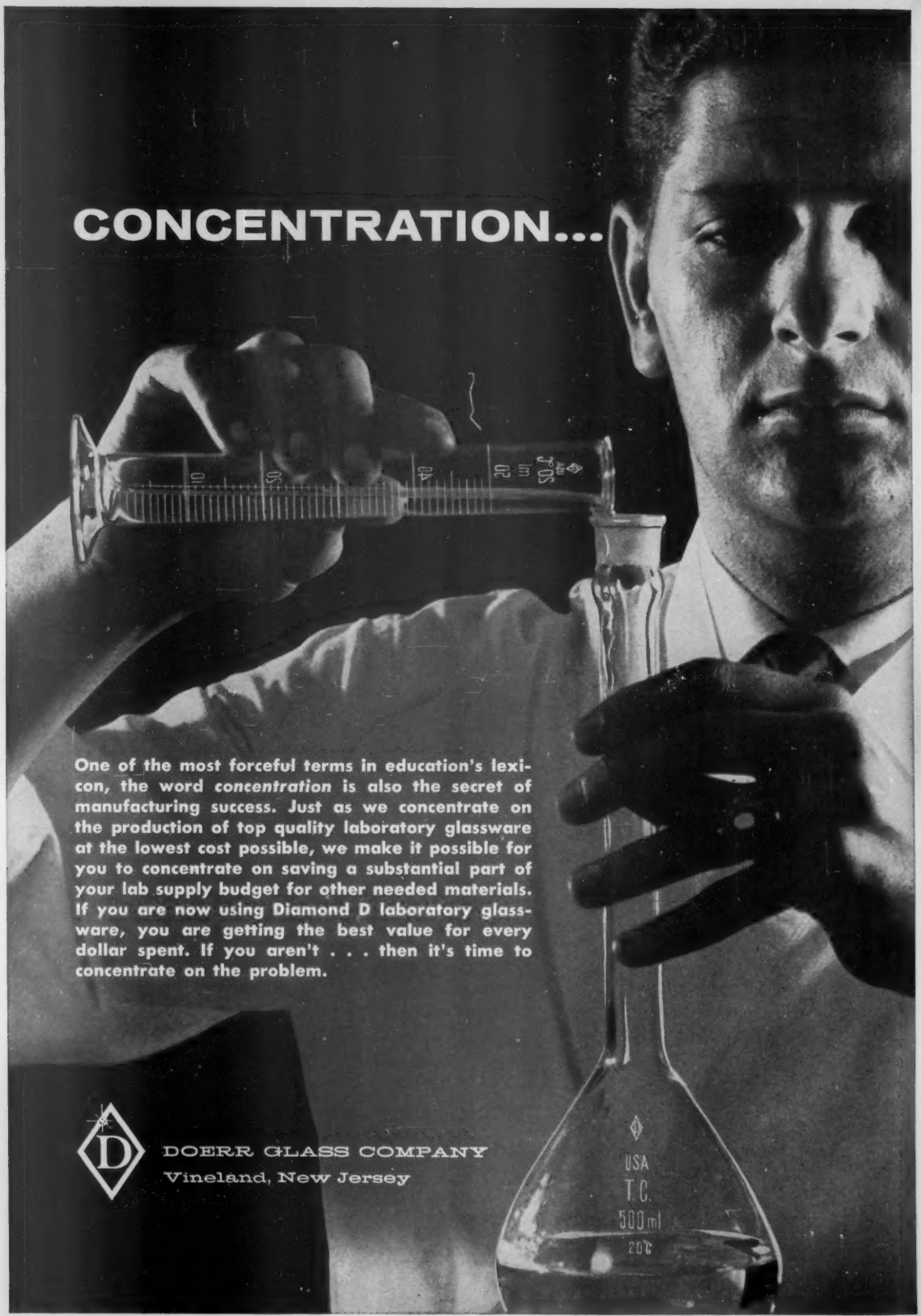
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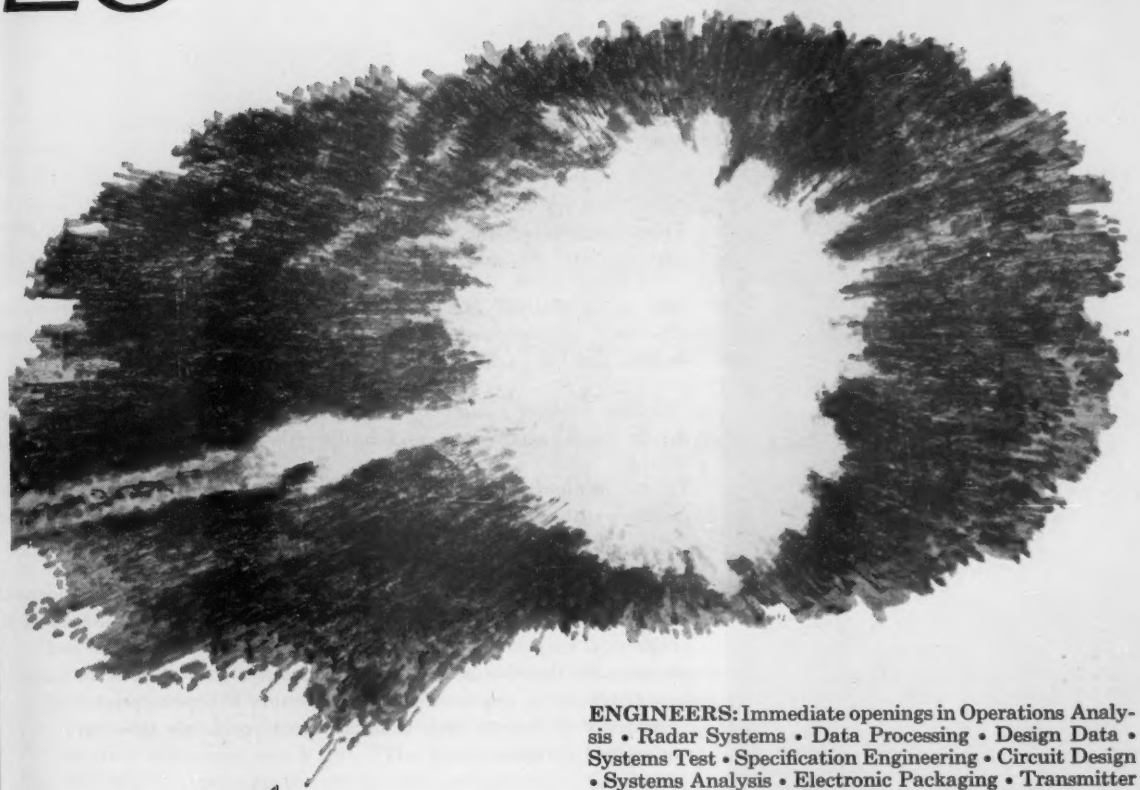

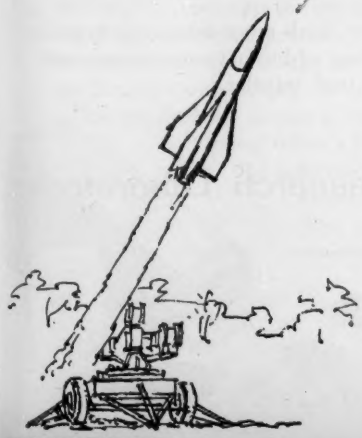

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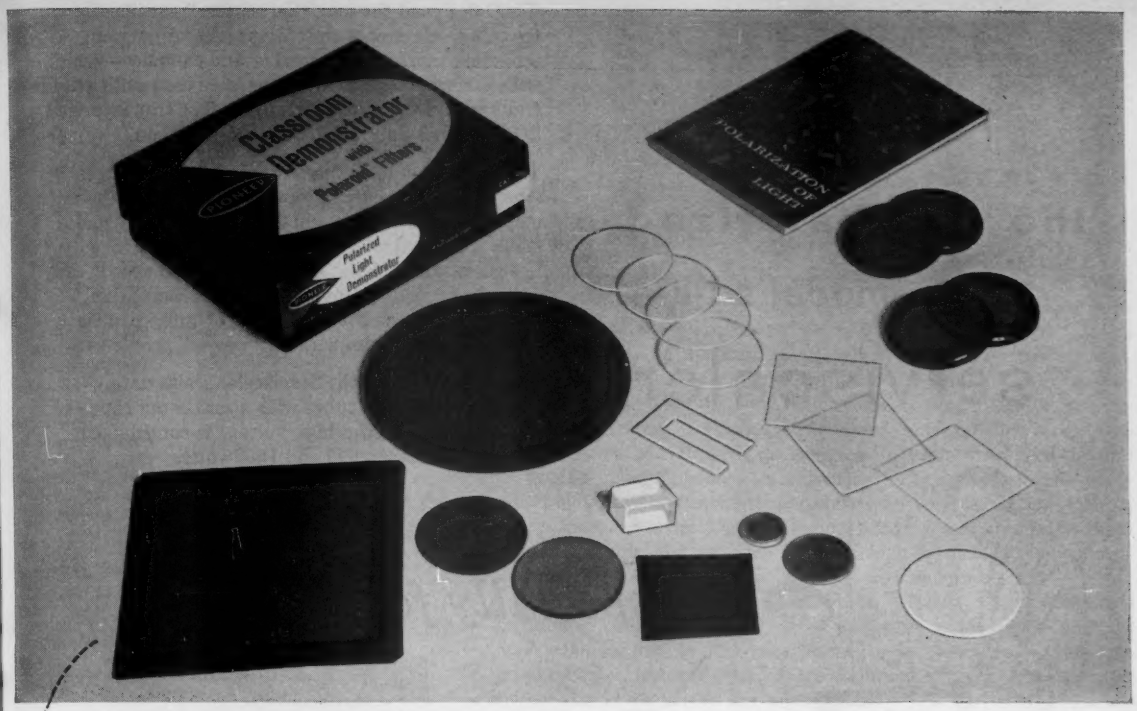

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Towards Regulating Cosmic Traffic

It will not spoil the story to say that at the close of Allen Drury's best selling novel, *Advise and Consent*, the Russians have just landed on the moon and claimed it in the name of the Soviet Union. But even as the Russians are planting a No Trespassing sign, the Americans have departed earth and are on their way to dispute the claim. This piece of fiction has the ring of truth, not because the great powers are destined to clash on the moon—the United States and the Soviet Union have each disavowed interest in owning celestial bodies—but because outer space presents many other, and more immediate, opportunities for conflict. Recent achievements in space technology, including bringing satellites safely back to earth, show the urgency for reaching agreements to control military uses of space and regulate peaceful uses.

Last December the United Nations established a permanent Committee on the Peaceful Uses of Outer Space to explore scientific and legal matters in this area. But like its predecessor, the temporary United Nations Outer Space Committee, which was boycotted by the Soviet Union on the ground that it was controlled by a majority from the West, the new, permanent committee has been plagued by questions of parity in East-West representation. Agreement was reached on the composition of the new committee. It is composed of seven members from the Soviet bloc, 12 from the West, and five from neutral countries, with the chairman to be selected from a neutral country. But some neutrals are more neutral than others, and a dispute arose over who was to be chairman and from what country he was to be selected.

One of the objectives of the Committee on the Peaceful Uses of Outer Space is to hold an international scientific conference, somewhat along the lines of the 1955 Geneva conference on the peaceful uses of atomic energy. Although the conference was originally proposed by the Soviet Union, the United States responded with such ardor that the Soviets may have grown suspicious about American intentions and decided to proceed cautiously. If the Americans had had their way earlier this year, a two-week conference would be winding up right now. Instead, in the matter of the conference as in that of the committee, proceedings have bogged down in a dispute over who is to be presiding officer. The Soviets wanted a Russian to be secretary general of the conference, while the United States wanted Dag Hammarskjold to appoint a citizen of a neutral country to the office.

Recently matters regarding the international control of space have begun to look a little brighter, at least in the United Nations space committee and the proposed scientific conference. Informed sources say the United States and the Soviet Union have resolved, or are about to resolve, their differences over the nationality of the presiding officers. The arrangements are to be made in the same spirit as that found in the composition of the committee itself. Settling questions of parity in East-West representation in space activities has been something of a struggle. We hope that at the forthcoming scientific conference on space the concern of governments over parity in management will be matched by an eagerness to share new scientific findings.—J.T.

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CURRENT PROBLEMS IN RESEARCH

Phenomena of the Solar Atmosphere

Astrophysicists are taking a new and more careful look at the outer layers of the sun.

R. Grant Athay

Astrophysicists long ago accepted the idea that because stars radiate energy into outer space, they cannot be in strict thermodynamic equilibrium. At the same time, however, they argued that the energy loss during a period of time characteristic of the collisional and radiative interactions was trivial in comparison to the energy density, and that, because of this, the stars could be considered to be in "local thermodynamic equilbrium," even in their surface layers. By this, they meant that Kirchoff's radiation laws were still applicable: the ratio of emissivity to absorptivity could be equated to the Planck function; absorption lines were formed in a cool stratum overlying a hotter surface; and, conversely, emission lines were formed in a relatively hot overlying stratum.

Parallel to this concept of local thermodynamic equilibrium were the postulates of radiative and hydrostatic equilibrium. According to these postulates, energy was transported outward by a radiative flux only, and the gaseous strata were supposedly quiescent.

The severest astrophysical test of these assumptions is found in the sun, which through its relative proximity allows a detailed study of its surface layers. Furthermore, the fortuitous phenomenon of a total eclipse of the sun provides a powerful means for studying the sun's external atmospheric layers. Two basic observations of the sun cast doubt on the foregoing postulates. On the one hand, the photospheric surface layers exhibit a granulation resembling convection cells, and on the other hand, the atmosphere overlying the photosphere extends to far greater heights than are compatible with the basic assumptions. Immediately above the photosphere and extending outward some 10,000 kilometers lies an irregular layer, known as the chromosphere because of its rosy-red color. This, in turn, gives way to a pearly-white corona often extending millions of miles into space.

Quite naturally, the first modification astrophysicists made in this classical picture, and the one most widely adhered to today, was postulation of the existence of a set of convection cells or "turbulent" eddies, which could explain both the granulation and the overly distended atmosphere. In the photosphere the convection cells have low velocity and do not seriously disturb the hydrostatic equilibrium. This is not so in the higher layers, however, and thus hydrostatic equilibrium has been dropped as an assumption. The assumptions of local thermodynamic equilibrium and radiative equilibrium are retained. The possibility that a coupling of the macroscopic motions to the microscopic degrees of freedom requires departures from these latter two assumptions is largely ignored, even though the macroscopic motions necessary to support the outer asmosphere are, of necessity, supersonic and might be expected to couple strongly with the microscopic degrees of freedom.

The startling discovery that resulted from the work of Grotrian and Edlén, that the mysterious emission lines in the spectrum of the corona are due to common elements in advanced stages of ionization, requiring temperatures of the order of a million degrees, forced a further reappraisal of the classical postulates. Both local thermodynamic equilibrium and radiative equilibrium were found to be invalid and were abandoned as totally unacceptable assumptions for the corona. Curiously, however, this new approach reestablished hydrostatic equilbrium as acceptable, because the high temperature was sufficient to explain the distended corona. Only recently have refined observations proved this assumption to be untenable. We are now forced to drop all three of the classical postulates.

Further down, in the chromosphere, the picture was not so clear. The assumptions of local thermodynamic equilibrium and of radiative equilibrium were retained with dogged determination. Supersonic turbulence was allowed to support the chromosphere but not to meddle with the internal degrees of freedom. Bit by bit this picture has crumbled as a result of improvements in eclipse data and the increasing evidence, growing mainly out of the work of R. N. Thomas and his co-workers, in support of large departures from local thermodynamic equilibrium in the chromosphere.

Solar physicists working with chromospheric problems almost universally recognize that such departures from the classical model of a stellar atmosphere exist. They are sharply divided on the degree and extent of the departures, however. Most of the standard refer-

The author is associate director of the High Altitude Observatory, University of Colorado, Boulder.

ence books in solar physics (*1*) adopt the point of view that departures from local thermodynamic equilibrium and radiative equilibrium are of secondary importance, whereas others, of a different school of thought (*2*, *3*), consider them as being of primary importance. The evidence in favor of the latter group appears overwhelming at this time. Again, those who are willing to abandon entirely the assumptions of local thermodynamic equilibrium and radiative equilibrium find that hydrostatic equilibrium is restored in the low chromosphere and that the assumed supersonic turbulence is replaced by high kinetic temperature (≈50,000°) in a large part of the chromosphere (*4*).

Many astrophysicists, today, argue that these large departures from the classical stellar atmosphere pertain to the outer atmosphere only; that they do not influence the photospheric layers; and that they do not play a significant role in the formation of the Fraunhofer spectrum. On the answer to these questions hinge the important questions of the chemical abundances of the elements and of stellar models.

In the sun, the stronger Fraunhofer lines, such as the early Balmer lines and the H and K lines of Ca^{II} are known to be formed in the photosphere. If our current concept of chromospheric models (*2*) is correct, at least in principle, then these stronger lines are formed at kinetic temperatures well in excess of the photospheric temperature. Kirchoff's laws, as applied to these lines, are, therefore, totally invalid. Absorption lines can and do appear in a high-temperature atmosphere overlying a relatively cool radiating surface.

Recently, J.-C. Pecker (*5*) has demonstrated that marked departures from local thermodynamic equilibrium very probably exist in the fainter Fraunhofer lines of Ti^{II}, and that the accepted solar abundance of titanium is probably inaccurate by as much as a factor of 5. Analyses are in progress for other elements, which will undoubtedly yield new evidence on this important question. The work is tedious, but rewarding. It is inherently difficult, since the grossly simplifying assumptions of local thermodynamic equilibrium cannot be made. The most serious difficulty, however, is the lack of atomic optical and collisional cross sections for calculating rate coefficients.

Important new observational techniques are contributing to the rapidly changing concepts of solar phenomena. I shall mention them only briefly here and in the discussions that follow. The development of magnetographs for mapping magnetic fields in the solar surface layers, as pioneered by H. W. and H. D. Babcock, is a striking example of these new techniques. Radio astronomy is contributing heavily. Rocket observations of the solar ultraviolet spectrum, as pioneered by W. A. Rense, R. Tousey, and H. Friedman, are providing invaluable data that were not available to earlier generations. Balloon-borne telescopes flown by D. E. Blackwell and A. Dollfus in Europe and by M. Schwarzschild in this country are photographing the solar granulation in greater detail than ever before. The coronagraphs developed by B. Lyot for producing artificial eclipses continue to provide new insight on coronal and chromospheric problems.

Interpretation of Solar Activity

The solar physicist's concept and interpretation of the transient phenomena of solar activity has, to a large degree, paralleled his concept of the normal solar atmosphere. The initial interpretation of a dark feature on the solar disk was that it is relatively cool, and of a bright feature, that it is relatively hot. Now, one must analyze the problem more carefully before giving such an answer. For features observed in the continuous spectrum, the initial interpretation is undoubtedly the correct one. For those observed in the monochromatic light of the stronger Fraunhofer lines, it may be totally incorrect. A dark feature in a Hα image of the sun, for example, is just as likely to be hot relative to the gases seen in the background radiation as it is to be cool. Similarly, a bright region, such as a flare, is not necessarily hotter than a dark region; it may simply be more dense.

These violations of Kirchoff's laws result, of course, from the failure of the conditions of thermodynamic equilibrium. Whether an object appears bright or dark in a given spectral line depends upon the balance between the various collisional and radiational transition rates influencing the number of electrons in the two energy levels involved in the formation of the line. Since the transition rate for collisions depends upon density as well as upon temperature, changes in either may affect the line formation. In thermodynamic equilibrium, this is not the case. Only the temperature may influence the radiation field.

Because of the complexity of the problem and the lack of adequate data, the solar physicist relies heavily upon the empirical approach as well as the analytical. As a case in point, consider the problems posed by solar flares.

Flares are perhaps the most vivid manifestation of solar activity, both as directly observed on the sun and as indirectly observed through their associated terrestrial effects. They brighten explosively and often produce associated effects at remote points on the sun, as well as on the earth. Because of departures from local thermodynamic equilibrium in flares, the interpretation of the spectrum is difficult. A number of evidences suggest that a flare is of higher density than its surroundings. There is, however, no unambiguous evidence that it is also hotter. The temperature of flares that occur at coronal heights, which are not infrequent, is certainly lower than that of the surrounding corona. And it may well be that flares occurring in the chromosphere are also cooler than their surroundings. The question has not yet been answered, however, in anything approaching a satisfactory manner.

There is abundant empirical evidence that flares are often, if not always, accompanied by the ejection of fast streams of protons and electrons from the flare itself or from nearby regions. Evidence for this, at the solar end, comes from the observed ejection of matter in the form of surge and spray prominences, from associated bursts of radio noise, and from the associated observed effects at some distance from the flare, such as the momentary vanishing of prominences recently observed by Moreton (*6*) (see Fig. 1). The radio noise bursts are of different types, but each type is believed to result from localized macroscopic motions of the solar plasma. In the cases of the so-called "fast" and "slow" drift bursts, the motion of the source can be followed directly with high-resolution radio telescopes.

Flares are also accompanied by flashes of ultraviolet and x-radiation. The evidence for this comes almost entirely from their effects on the ionosphere but a few direct observations of the waning stages of flares from rockets by Friedman (*7*) and his co-workers have confirmed this conclusion. However, no direct observations are as yet

available for the early or maximum phases of a flare, where the greatest changes are expected to occur.

If each flare were a repetition of every other flare, our problem would still be complex, but greatly simplified. Instead, each flare seems to be somewhat different. Some large flares produce ionospheric disturbances, geomagnetic storms, auroral displays, and increases in cosmic rays, all of major amplitude. Other flares that show similar characteristics in their visual spectrum produce small or undetected terrestrial effects. At the same time, what appear to be minor flares, from visual observation, may produce major terrestrial responses. These facts forcibly remind us that what we observe with our telescopes and spectrographs—for example, the brightening of certain spectral lines—is not related in a simple way to the physical processes in the flare phenomenon.

The great disadvantage in the empirical approach to the understanding of flare phenomena lies in its ambiguity. Does a flare itself eject clouds of matter from the sun? Does it emit intense bursts of ultraviolet and x-radiation? The answer to each of these questions might well be no. Instead, each of these separate manifestations of something unusual on the sun may be symptomatic of a larger, more widespread phenomenon that we have not

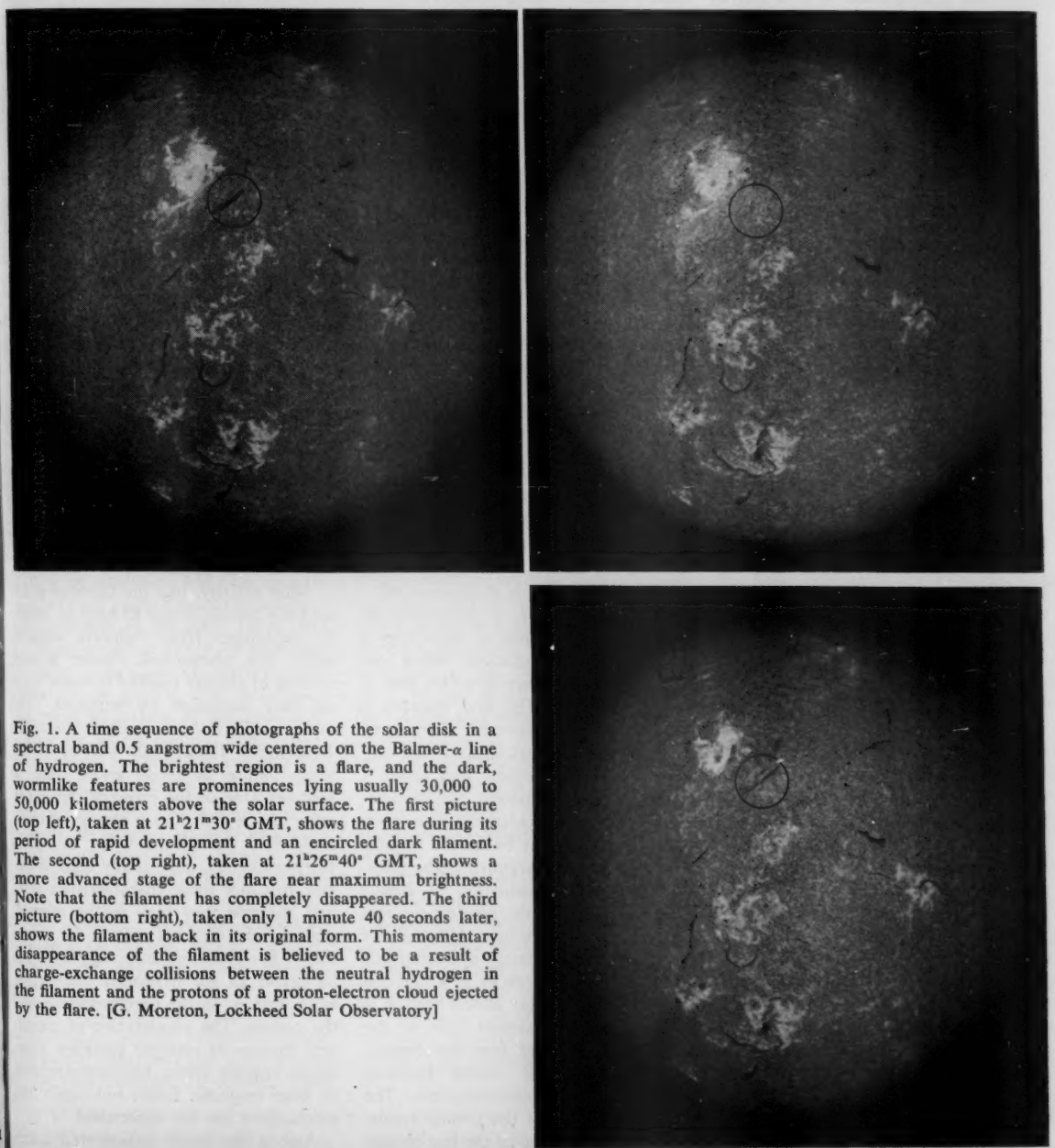

Fig. 1. A time sequence of photographs of the solar disk in a spectral band 0.5 angstrom wide centered on the Balmer-α line of hydrogen. The brightest region is a flare, and the dark, wormlike features are prominences lying usually 30,000 to 50,000 kilometers above the solar surface. The first picture (top left), taken at $21^h21^m30^s$ GMT, shows the flare during its period of rapid development and an encircled dark filament. The second (top right), taken at $21^h26^m40^s$ GMT, shows a more advanced stage of the flare near maximum brightness. Note that the filament has completely disappeared. The third picture (bottom right), taken only 1 minute 40 seconds later, shows the filament back in its original form. This momentary disappearance of the filament is believed to be a result of charge-exchange collisions between the neutral hydrogen in the filament and the protons of a proton-electron cloud ejected by the flare. [G. Moreton, Lockheed Solar Observatory]

yet observed in its broader aspects. There is some evidence that this is indeed the case. However, we cannot be certain until we understand the basic mechanisms involved in flare phenomena and the thermodynamic conditions within the flare itself. At present, we can do little more than speculate about the answers to these problems.

Indices of Solar Activity

The geophysicist in his search for the terrestrial consequences of solar activity turns to the solar physicist for a so-called "index" of solar activity, with the plea that it be "objective." What he actually wants is an index of corpuscular or short-wavelength electromagnetic radiation, which, of course, the solar physicist cannot supply "objectively" because he has no clear idea just what he should be objective about. To be sure, he can be objective about counting or measuring areas of sunspots or flares, but he cannot yet be objective in his use of such values, once they are obtained. Should he, for example, combine the area of a flare with its brightness, or would he get a better index by ignoring both of these aspects and taking the width of a spectral line as an index? The answer is simply that he doesn't know, nor will he know until the flare is understood as a physical phenomenon.

One may invert this reasoning and argue that we will not understand flares until we discover and understand the empirical associations between flares and other observable phenomena. To an extent this is true. However, it seems clear that the most straight-forward approach to the problem is to first arrive at a correct interpretation of the optical spectrum, which should at least furnish a thermodynamic description of the flare we are trying to explain. Once we have such a description, solution of the associated problems should be much simpler.

I do not intend by this discussion to discourage those who use solar indices in empirical studies of solar and solar-terrestrial phenomena. On the contrary, such studies are of great value and have proved to be fruitful. These studies are handicapped, however, by the unanswered questions concerning the basic meaning of solar indices. When physically meaningful indices, in terms of emitted solar radiation, are available, we can expect accelerated progress from sun-earth researches.

Origin of Solar Atmospheric Phenomena

The existence of the solar chromosphere and corona is a manifestation of a widespread departure from radiative equilibrium in the external layers of the sun. The origin and cause of this departure is as yet unspecified. Intuitively, one feels that it must, in some way, be associated with the hydrogen convection zone lying just below the photosphere. The outward decrease in temperature in the solar interior eventually reaches a value where protons and electrons start to recombine to form neutral hydrogen. When this occurs, the opacity increases sharply and the temperature gradient becomes steeper. As a result, the gases become convectively unstable and transport part of the energy flux by convection currents and part by radiation. It is presumably this convection that produces the photospheric granulation.

The convection zone very probably has side effects such as the generation and propagation of sound waves and hydromagnetic waves. Because of the outward decrease in density, certain of these waves tend to amplify and accelerate as they propagate outward. Eventually, they dissipate their energy into the propagating medium and effect a transfer of energy from the interior layers to the exterior layers by nonradiative processes. This energy, in turn, must be disposed of by the medium. Two questions stand out at this point in the theory. Where is the energy dissipated and how is it disposed of?

The last question is apparently the easier to answer. Most of the energy is disposed of by radiation, which can now escape into outer space because of the low opacity. The first question is not so readily answered. For many years we argued that because the temperature of the corona was about 1 million degrees, whereas the chromosphere was much cooler, the main energy dissipation must be in the corona. This is a fallacious argument. The greatest nonradiative dissipation of energy must occur where the greatest net loss of radiant energy occurs. Although we have not yet completely evaluated this net loss of radiant energy as a function of height in the solar atmosphere, it is clear that the maximum falls in the chromosphere rather than the corona. In this sense, the corona becomes secondary to the chromosphere. The high temperature in the corona results from the need to ionize the less abundant heavy elements several times before they can radiate efficiently enough to dispose of the energy dissipated therein. The chromosphere, on the other hand, disposes of its energy by radiation from the more abundant elements, hydrogen and helium, which radiate most efficiently at temperatures of some tens of thousands of degrees.

In the chromosphere we observe another phenomenon that probably plays a vital role in the energy cycle. This is the phenomenon of spicules, which are high-speed jets of gases ejecting upwards from the chromosphere into the corona (see Fig. 2). They occur more or less uniformly over the entire sun and number about 10^5 at any one time. Their diameter is of the order of 500 kilometers, and their outward elongation usually exceeds 10,000 kilometers during their few minutes of life. In a period of 24 hours, most of the chromospheric surface undergoes a spicule upheaval and the outward flux of matter is sufficient to replace the entire corona several times.

Spicules apparently originate in the low chromosphere, where the maximum net energy loss by radiation seems to occur, though we are somewhat uncertain as to the amount of this loss. The causes and effects of spicules are largely unknown. Nevertheless, it seems clear that they point to a basic instability in the chromospheric plasma. Their relationship to the energy balance of the chromosphere and corona presents one of the most challenging problems of solar physics.

Solar activity, like the chromosphere and the corona, seems to have its origin in departures from radiative equilibrium. The phenomena cluster around centers of activity marked in some stage of their evolution by sunspots. The active regions show much evidence of instabilities. These might well be of the same nature as those giving rise to spicules. One of the primary characteristics of active regions is abnormal magnetic field strength and, for many of the phenomena, abnormally high densities. Just how these characteristics arise and the role they play in the over-all phenomena are not clear. It seems likely, however, that solar activity owes its origin to the hydrogen convection zone, just as do the chromosphere and the corona. The acceleration of clouds and streams of charged particles from active regions seems inevitably related to their magnetic fields, but again the mechanisms are not understood.

Among the many unanswered ques-

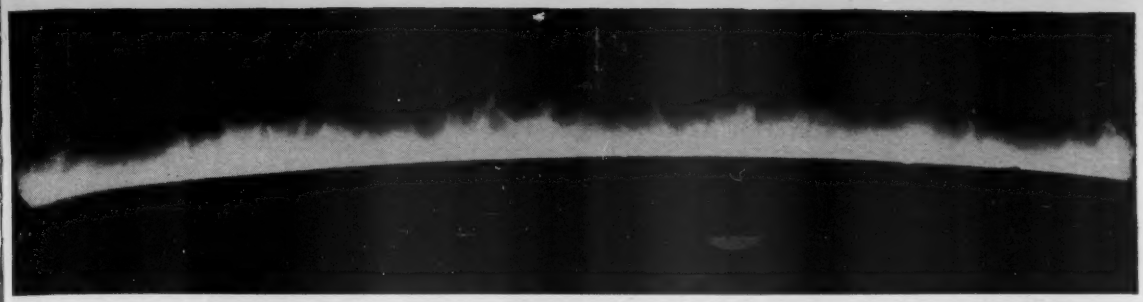

Fig. 2. A photograph of an artificial eclipse, showing the chromosphere at the extreme edge of the sun in the light of the Balmer-α line of hydrogen. The fine, bristle-like features are spicules. Their mean diameter is less than 1 second of arc, which is 725 kilometers at the distance of the sun. [R. B. Dunn, Sacramento Peak Observatory]

tions regarding phenomena of the solar atmosphere, those that stand out are: (i) What is the mechanism of energy supply? (ii) What is the role of magnetic fields? (iii) What is a flare? (iv) What are the instabilities that give rise to spicules and solar activity? (v) What are the nature and variability of solar corpuscular and short-wavelength radiation incident upon the earth as a result of solar activity?

New Horizons

Several recent developments have opened new horizons in solar and solar-terrestrial physics and promise to aid greatly in providing answers to some of the foregoing questions. Not the least of these is the recognition among solar physicists that they must consider departures from local thermodynamic equilibrium to be significant and that, as a result they need vastly more data than have been available in the past. These data are being gathered partly by extending the older techniques of observing and partly by developing new techniques. The most important of the new techniques are extraterrestrial observations from rockets and satellites and mapping of the magnetic fields in the surface layers of the sun. Among the extensions of older techniques are the high-speed photographic patrols of the solar disk, and of the chromosphere and corona at the limb, and the re-emphasis on eclipse observing to obtain detailed information on the spectrum of the upper photosphere and the lower chromosphere.

The extraterrestrial observations are furnishing immensely valuable information in the ultraviolet and x-ray regions of the electromagnetic spectrum. These observations will tell us much concerning the basic structure of the chromosphere and corona, provided we can obtain from laboratory experiments the necessary optical and collisional cross sections that are essential to a reasonable interpretation of the data. At these wavelengths the solar spectrum is characterized by a faint continuum crossed by numerous strong emission lines. A naive interpretation of these lines, following the concepts of local thermodynamic equilibrium, would indicate temperatures of about 7000° where the central core of the Lyman-α line of hydrogen is formed and about 20,000° where the corresponding line of ionized helium is formed. A more carefully considered treatment, however, indicates that the temperatures are actually of the order of 50,000° to 100,000° in these regions (*8*).

In the Lyman-α line of hydrogen the brightness of the solar surface is highly irregular, with most of the flux originating in active regions (*9*). Details of this sort are not yet available for other emissions in the short-wavelength regions of the spectrum, but there is little doubt that they will show similar concentrations in active regions. The total flux below about 1800 angstroms probably undergoes strong variations during the sunspot cycle and with the appearance of certain features of solar activity.

Corpuscular radiation from the sun is likewise being closely studied from satellites and from stratospheric balloons. A great deal has already been learned concerning this difficult problem, too much to summarize here. As in the case of the ultraviolet and x-ray spectrum, however, we are just at the threshold of challenging new discoveries.

In the meantime, much is being done from ground and from balloon-borne observatories. Observers in the United States, Russia, and England are carefully and painstakingly mapping the solar magnetic fields, hoping to establish their relationships to such phenomena as flares and other forms of solar activity. Observers the world over are constantly studying the solar disk, from high-altitude balloons, from mountain tops, and from sea level. Those with artificial eclipse-producing coronagraphs are trying to unravel the mysteries of the corona, of spicules, and of the upper chromosphere. Some (in many cases members of these same groups) are busy chasing eclipse shadows to remote regions of the earth, where, because of the eclipse produced outside of our terrestrial atmosphere, they find unparalleled observing conditions and opportunities for studying some of the vital solar problems.

Solar physics is making rapid advances at a constantly accelerating rate. Each recent decade has seen many revisions of concepts and many new data. The coming decade promises even greater progress.

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Radiocarbon from Nuclear Tests, II

Future concentrations predicted for this isotope in the earth's carbon cycle suggest its use in tracer studies.

Wallace S. Broecker and Edwin A. Olson

In the testing of nuclear weapons to date, large amounts of radiocarbon have been synthesized and injected into the earth's carbon cycle, specifically into the atmosphere (*1–7*). From this reservoir, such radiocarbon makes its way into other carbon reservoirs through numerous mixing processes, with the result that its concentration at various places in the carbon cycle is rising. Thus, scientists are in possession of a large-scale tracer that potentially can elucidate the mechanisms and rates of many natural processes that involve the element carbon. It is the purpose of this article (*8*) to point out several potential tracer applications of bomb-produced carbon-14. In order to evaluate these tracer possibilities it is necessary to have an estimate of the total amount of bomb-produced C^{14} and to know its present as well as its future distribution in the carbon cycle. Such estimates are given below, along with an evaluation of the experimental data used as the basis for the inventory estimates.

Reliability of Vegetation as an Indicator of Atmospheric Levels

In a previous report (*4*) on radiocarbon produced during nuclear tests, by Broecker and Walton, it was noted that the C^{14}/C^{12} ratios in tree-leaf samples collected in Palisades, New York, were significantly lower than those in samples collected at the same time in other areas. In order to determine the reason for this difference, a series of carefully chosen samples collected in the Palisades area during the late spring of 1959 were analyzed. Samples of atmospheric CO_2, leaves, and soil CO_2 were analyzed. The samples of atmospheric CO_2 were collected by exposing pans of "CO_2-free" KOH solution to the atmosphere; the samples of soil CO_2 were collected in a similar way except that the pans were set under an inverted, open-top oil drum driven into the soil. As determined from the quantity of CO_2 collected, the escape rate of CO_2 from the soil was approximately 7 gram molecules per square meter per month. It should be emphasized that this rate depends on both the time of year and the type of soil; (in this case the time was late spring and the soil was of the great soil group, Sol Brun Acide, which has here a humic-rich zone down to about 8 inches and, below that, a yellow subsoil).

The analytical results are given in Table 1, where they are expressed in ΔC^{14} units (*9*) (that is, per-millage difference between the fractionation-corrected value for radioactivity of the sample and 0.95 times the activity of the National Bureau of Standards oxalic acid C^{14} standard). Values for samples grown just before bomb-testing began range from −20 to −40 per mill on this scale, and age-corrected values for samples of pre-1900 woods are close to zero.

The two samples of leaves taken from a forest in Palisades (L528A and L528E) have ΔC^{14} values about 50 per mill lower than a sample of leaves collected at the same time in Carson City, Nevada (L516E), a difference consistent with findings in earlier comparisons between Palisades and Great Basin samples (*4*). The difference cannot be explained by a uniformly lower C^{14}/C^{12} ratio in the air of the Palisades area because values for two samples from a nearby area agree well with the findings for Carson City leaves. One of these samples (L528B) was air-collected atop a 400-foot tower in Alpine, New Jersey, and the other (L528F) was a leaf sample from a lone tree on a golf course at Rockleigh, New Jersey. Furthermore, the low values for the two forest samples cannot be attributed to incorporation by the growing leaves of soil CO_2 which had accumulated in the forest air, since a sample of atmospheric CO_2 (L528C) taken within 3 feet of the growing leaves had a value 57 per mill higher than the average for the two leaf samples and had the same ratio as the CO_2 collected atop the 400-foot tower.

Three possible explanations remain: (i) the leaves photosynthesized most of their carbon prior to air sampling, at a time when the Palisades area had a considerably lower atmospheric C^{14}/C^{12} ratio than it had at the time leaves were collected; (ii) the leaves of certain trees receive a major fraction of their carbon from organic compounds stored for significant periods of time in the tree roots; or (iii) the forest leaves received about 30 percent of their carbon from soil CO_2 taken in by their roots. All three explanations obviously pose the problem of why the Rockleigh sample, taken only 2 miles west of the Palisades samples, does not show a similar effect. Since the value for Rockleigh leaves matches the value for the atmosphere and since none of the leaves in the Palisades area sprouted until early May, the first explanation given above is very unlikely. It is also difficult to believe that in one tree (a maple) a large fraction of the carbon incorporated in leaves came from stored organic materials while in another (a black cherry) only a small fraction came from this source.

The third explanation is qualitatively consistent with work cited by Rabinowitch (*10*), but whether it is quantitatively possible is another question. Tracer experiments by Kursanov *et al.* (*11*) indicate that as much as 20 percent of the carbon photosynthesized by bean seedlings can be introduced through the roots. However, further work, by Stolwijk and Thimann (*12*) casts some doubt on this high percentage. Whether under certain circumstances tree roots can provide a third of all photosynthesized carbon is at present not known.

If appreciable uptake of soil CO_2 through the roots of certain trees is

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accepted as the true explanation, there remains the problem of explaining why significant uptake from the soil has occurred in the case of the Palisades samples and not in the case of the Rockleigh samples. A possible explanation based on the different growth environments is as follows: The Rockleigh tree grows in an open area and in one where deep root penetration is possible; on the other hand, the Palisades tree grows in a densely vegetated area where there are only 2 to 4 feet of soil overlying bedrock (Palisades diabase). Thus, the tree in the Rockleigh area may take up less soil CO_2 because (i) there is less decaying vegetation and hence less soil CO_2 available and (ii) a smaller percentage of its roots are concentrated near the zone of maximum soil CO_2 production.

Certainly more evidence must be obtained before this problem can be solved completely. It can only be said at this point that there is almost certainly a real lag between the C^{14}/C^{12} ratio in leaves from the Lamont Observatory forest and the ratio in the atmosphere. Even though this may be an unusual case, it shows that monitoring of atmospheric CO_2 is most reliably accomplished through direct collection of atmospheric CO_2. Since only a few air samples were taken in the Northern Hemisphere prior to 1958, results on vegetation are the only source of information for this period. Their correct evaluation is hence a matter of importance. The information available indicates that vegetation samples are generally reliable indicators of atmospheric C^{14}/C^{12} ratios but that they may occasionally provide only minimal estimates.

Table 1. Data relating to the reliability of vegetation samples as indicators of atmospheric C^{14}/C^{12} ratios.

Collection date	Material	Location	Physical surroundings	δC^{14}	δC^{13}	ΔC^{14}	Sample No.
5/16/59 to 6/7/59	Atmospheric CO_2	Alpine, N.J. 40°57′N, 73°55′W	Top of 400-ft radio tower	251 ± 6	−18.9	249 ± 6	L528-B
5/16/59 to 6/7/59	Atmospheric CO_2	Palisades, N. Y. 41°00′N, 73°55′W	3 ft above ground in dense forest	264 ± 7	−16.9	257 ± 7	L528-C
6/5/59	Willow leaves	Carson City, Nev. 39°07′N, 119°53′W	5 ft above ground in a clump of willows	249 ± 8	−23.8	259 ± 8	L516-E
6/9/59	Oak leaves	Rockleigh, N.J. 41°00′N, 73°58′W	On knoll in middle of golf course	232 ± 8	−25.8	246 ± 8	L528-F
5/16/59	Maple leaves	Palisades, N.Y. 41°00′N, 73°55′W	5 ft above ground on small tree in dense forest	178 ± 6	−25.9	189 ± 6	L528-A
6/7/59	Maple leaves	Palisades, N.Y. 41°00′N, 73°55′W	5 ft above ground on small tree in dense forest	198 ± 7	−26.2	211 ± 7	L528-E
5/16/59 to 6/7/59	Soil CO_2	Palisades, N.Y. 41°00′N, 73°55′W	Leaf-matted ground in dense forest	106 ± 7	−23.2	108 ± 7	L528-D

Bomb-Produced Carbon-14 Inventory

Additional samples defining the tropospheric C^{14}/C^{12} ratio extend Broecker and Walton's curve (*4*) from March 1958 to June 1959. The additional data, given in Table 2 and Figure 1, show that the rate of rise of the C^{14}/C^{12} ratio has increased in both hemispheres—from 5 up to 11 percent per year for the Northern Hemisphere and from 4 up to 7 percent per year in the Southern Hemisphere. At the same time, the ratio of the two hemispheric bomb effects has remained about the same (that is, $C^{14}/C^{12}_{South} = 0.67\ C^{14}/C^{12}_{North}$).

Data published by Willis (*2*) and by Tauber (*3*) suggest that the tropospheric ΔC^{14} value for northern Europe rose to about 300 by 1 July 1959. On the other hand, Hayes *et al.* (*5*) have analyzed lemon grass grown in low latitudes of the Northern Hemisphere and find that, as of June 1959, there has been a 26.8 percent rise over prebomb levels. This is equivalent to about 250 per mill on the ΔC^{14} scale and agrees well with data given in this article. From the above differences it appears that true geographical variations do exist within the troposphere of the Northern Hemisphere, presumably the result of finite mixing rates for new C^{14} added from the stratosphere. Munnich and Vogel (*13*) have also demonstrated rather marked seasonal oscillations within the Northern Hemisphere, which are apparently related to the spring maximum of stratospheric Sr^{90} fallout (*14*). On the basis of an average Southern Hemisphere ΔC^{14} value of +160 in June 1959 and a Northern Hemisphere average of +250, the tropospheric inventory was 6.3×10^{27} atoms on 1 July 1959 as compared to 3.6×10^{27} on 1 July 1958. Because of inhomogeneities in the troposphere of the Northern Hemisphere, the 1959 estimate has an uncertainty of perhaps 15 percent.

Table 2. Data relating to the increase in the tropospheric C^{14}/C^{12} ratio between the fall of 1958 and the fall of 1959.

Collection date	Location	Material	δC^{14}	δC^{13}	ΔC^{14}	Sample No.
		Northern Hemisphere				
8/8/58 to 8/11/58	Mediterranean Sea 36°N, 7°E	Atmospheric CO_2	214 ± 8	−5.1	176 ± 8	L493-O
8/15/58 to 8/19/58	Atlantic Ocean 39°N, 30°W	Atmospheric CO_2	200 ± 8	−5.8	162 ± 8	L493-P
10/22/58 to 10/24/58	Atlantic Ocean 33°N, 76°W	Atmospheric CO_2	197 ± 5	−5.1	159 ± 5	L529-A
11/8/58 to 11/10/58	Atlantic Ocean 11°N, 76°W	Atmospheric CO_2	193 ± 8	−6.6	159 ± 8	L529-M
4/4/59	Tucson, Ariz. 32°N, 111°W	Tree leaves	193 ± 9	−20.7	193 ± 8	L523
5/16/59 to 6/7/59	Palisades, N.Y. 41°N, 74°W	Atmospheric CO_2	264 ± 7	−16.9	257 ± 7	L528-C
6/5/59	Carson City, Nev. 39°N, 120°W	Tree leaves	249 ± 8	−23.8	259 ± 8	L516-E
6/9/59	Rockleigh, N.J. 41°N, 74°W	Tree leaves	232 ± 8	−25.8	246 ± 8	L528-F
5/16/59 to 6 7/59	Alpine, N.J. 41°N, 74°W	Atmospheric CO_2	251 ± 6	−18.9	249 ± 6	L528-B
10/12/59 to 10/18/59	Alpine, N.J. 41°N, 74°W	Atmospheric CO_2	236 ± 6	−24.0	246 ± 6	L547-A
		Equatorial Region				
12/1/58 to 12/4/58	Pacific Ocean 1°N, 84°W	Atmospheric CO_2	176 ± 9	−6.1	140 ± 9	L537-A
		Southern Hemisphere				
4/5/58 to 4/18/58	Atlantic Ocean 35°S, 24°E	Atmospheric CO_2	118 ± 5	−5.7	82 ± 5	L493-L
4/28/58 to 5/2/58	Atlantic Ocean 27°S, 46°E	Atmospheric CO_2	125 ± 6	−5.4	87 ± 6	L493-M
5/13/58 to 5/17/58	Atlantic Ocean 13°S, 65°E	Atmospheric CO_2	120 ± 5	−3.6	78 ± 5	L493-N
1/28/59	Pacific Ocean 49°S, 75°W	Atmospheric CO_2	144 ± 7	−7.2	111 ± 7	L537-L
3/16/59	Lima, Peru	Grass	135 ± 7	−10.1	135 ± 7	L519-B

Table 3. Data relating to bomb-produced C^{14} concentration in surface ocean water (*24*).

Sample No.	Collection date	Location	δC^{14}	δC^{13}*	ΔC^{14}†	Difference from pre-bomb av.	
						Measured	Predicted
		North Atlantic (pre-bomb av., −49 ‰)(15)					
L465-Z	11/13/57	34°N,65°W	−1 ± 5	+1.5	−54 ± 8	−5	10
L464-B	11/16/57	30°N,62°W	26 ± 5	+4.3	−33 ± 8	16	10
L464-D	11/17/57	29°N,61°W	13 ± 6	−1.8	−33 ± 6	16	10
L464-O	11/26/57	15°N,40°W	1 ± 8	−1.0	−47 ± 8	2	10
L529-I	11/15/58	16°N,75°W	20 ± 7	+3.8	−38 ± 7	11	20
L538-I	5/14/59	10°N,45°W	19 ± 6	+4.1	−39 ± 7	10	27
L538-P	7/10/59	35°N,74°W	28 ± 8	+0.7	−23 ± 8	26	29
		South Atlantic (pre-bomb av., −58 ‰)(15)					
L464-R	12/5/57	1°N,33°W	−28 ± 6	−1.1	−76 ± 6	−18	10
L464-T	12/16/57	10°S,34°W	−8 ± 7	(−1.0)	(−56 ± 9)	2	10
L465-D	12/26/57	23°S,38°W	7 ± 7	−2.1	−39 ± 7	19	10
L465-X	4/6/58	34°S,18°E	−2 ± 5	+1.0	−54 ± 5	4	12
		South Pacific (pre-bomb av., −54 ‰)(7)					
R359	12/17/57	41°S,174°W	15 ± 5	(−2.0)	(−31 ± 5)	23	11
R388/1	3/4/58	36°S,174°E	1 ± 9	(−2.0)	(−45 ± 9)	9	12
R376	3/5/58	41°S,174°E	0 ± 5	(−2.0)	(−46 ± 5)	8	12
R388/3	3/6/58	28°S,176°E	8 ± 6	(−2.0)	(−38 ± 8)	16	12
R388/5	3/8/58	21°S,177°E	9 ± 6	(−2.0)	(−37 ± 9)	17	12
R388/10	3/18/58	20°S,175°E	13 ± 5	(−2.0)	(−33 ± 5)	21	12
R388/11	3/19/58	21°S,173°E	13 ± 5	(−2.0)	(−33 ± 5)	21	12
R388/12	3/21/58	18°S,172°E	12 ± 5	(−2.0)	(−34 ± 5)	20	12
R388/15	3/23/58	15°S,178°E	12 ± 5	(−2.0)	(−34 ± 5)	20	12
R388/21	3/31/58	32°S,175°E	5 ± 5	(−2.0)	(−41 ± 5)	13	12

* Parentheses in column 5 denote estimates rather than measured values. † Parentheses in column 6 indicate that the ΔC^{14} value is based on an estimated δC^{13} value.

Table 4. Estimates of the distribution of bomb-produced C^{14} on 1 July for a number of different years (A.D.) (units, 10^{27} atoms).

Reservoir	Year								Steady-state distribution of natural radiocarbon
	1958	1959	1965	1976	2000	2100	2350	7520	
Strato-sphere	8.4*	8.0–17.0	3.1–6.4	1.1–2.4	0.6–1.0	0.2–0.3	0.1–0.2	0.0	7
Tropo-sphere	3.6	6.3	6.7–10.1	4.5–7.2	2.6–4.0	0.8–1.2	0.4–0.6	0.2	28
Biosphere	0.2	0.3	0.8–1.0	1.0–1.6	0.7–1.2	0.2–0.3	0.1	0.0	8
Ocean	0.8	1.4	5.4–7.5	9.4–13.8	12.0–18.7	14.5–22.7	14.4–22.6	7.8–12.3	1960
Total	13.0	16.0–25.0	16.0–25.0	16.0–25.0	15.9–24.9	15.7–24.5	15.0–23.5	8.0–12.5	2000

* Hagemann *et al.* (*6*).

Table 5. Parameters for reservoirs making up model dynamic carbon cycle.

Reservoir	Elevation range (m)	Contact area with troposphere (m^2)	Carbon content (gram molecules)	Steady-state ΔC^{14}		Mean residence time (yr)
				Measured*	Computed†	
Stratosphere	>14,000	510×10^{12}	11×10^{15}		+75	5
Troposphere	<14,000		46×10^{15}	0		4
Terrestrial biosphere			13×10^{15}			10
Pacific plus Indian Ocean (<50°S) surface water	<100	220×10^{12}	50×10^{15}	−32		8
North Atlantic (5°N–55°N) surface water	<100	40×10^{12}	9×10^{15}	−35		5
South Atlantic (50°S–5°N) surface water	<100	40×10^{12}	9×10^{15}	(−46)	−65	5
Pacific plus Indian Ocean (<50°S)‡ deep water	>100		1980×10^{15}	−200		800
Atlantic (50°S–55°N) deep water	>100		655×10^{15}	−104		650
Antarctic Ocean (>50°S)	Entire	45×10^{12}	420×10^{15}	(−113)	−120	70
Arctic Ocean (>55°N)‡	Entire	15×10^{12}	40×10^{15}	(−28)	−30	30

* Numbers in parentheses were not used in steady-state computations. See values derived from computations (col. 6) and compare with measured values. † The computation was based on the assumption that stratospheric mean residence time is 5 years and that half of the natural radiocarbon production is above the tropopause. ‡ Bering Strait isolates the Arctic Ocean from the Pacific.

On the basis of the same assumptions used previously (*4*), the amount of bomb-produced C^{14} stored in living organic materials is calculated to be 0.3×10^{27} atoms. Although the uncertainty in this estimate is large, bomb-produced C^{14} in the organic reservoir continues to be so small a part of the total inventory as to be unimportant.

The amount of bomb-produced C^{14} which has entered the oceans has also been computed, according to the method of Broecker and Walton (*4*) except that the return flux of bomb-produced C^{14} from the ocean to the atmosphere has been taken into account (in accordance with the circulation model discussed below). Based on a CO_2 exchange rate of 22 gram molecules per square meter per year (*15*), a value of 1.4×10^{27} C^{14} atoms is obtained for the 1 July 1959 oceanic inventory. It should be noted that this value is essentially independent of the oceanic mixing model selected (see *15*).

If all of this oceanic C^{14} had remained in the upper 100 meters of water, the C^{14}/C^{12} ratio of the surface layer should have risen 30 per mill between 1 March 1955 and 1 July 1959. According to the more elaborate model discussed below, which takes into account loss to deep-water masses, the average value for surface water should have risen 29 per mill by 1 July 1959. The corresponding increases for 1 July 1958 and 1 July 1957 are, respectively, 14 and 7 per mill.

Ocean-water measurements available to date are summarized in Table 3. Since the differences are small, it can only be said that they are consistent with the values predicted by the model. Future measurements should provide a good check on the circulation model adopted here, since the rise in surface ocean ΔC^{14} values will be such that measurement errors will not be so significant.

If the rate of addition of bomb-produced C^{14} to the troposphere is assumed to be linearly dependent on the stratospheric inventory, then the rising slope of the curve in Fig. 1 indicates a rising stratospheric inventory. Specifically, the rise from July 1958 to July 1959 is more than twice the annual increase for the previous two years; hence, Hagemann's average inventory (*6*), measured for the period 1955–1958 (namely, 7.2×10^{27} atoms), should be scaled up to 17×10^{27} atoms in order to obtain the stratospheric inventory on 1 July 1959. Much of this inventory increase is presumably due

to Russian tests during the fall of 1958.

A nearly identical estimate of the stratospheric inventory is obtained by taking Hagemann's estimate (*6*) of the total amount of C^{14} thus far produced by bombs (25×10^{27} atoms) and subtracting the amount of this C^{14} which had passed into the other reservoirs (troposphere, biosphere, and oceans) up to 1 July 1959. The latter amount is given below as 8×10^{27} atoms, and therefore the stratospheric inventory, by difference, is 17×10^{27} atoms.

Despite the agreement between these two estimates of stratospheric inventory, there is reason to believe that they may represent only an upper limit. As suggested by Martell (*16*), nuclear debris added to the polar stratosphere may exhibit a significantly shorter residence time than that added to the stratosphere of middle latitudes. Before July 1958 most bomb C^{14} was probably produced by testing in the equatorial Pacific, whereas subsequently the major part of the C^{14} was added by Russian testing in the Arctic. Thus, the increased slope may represent rapid transfer of C^{14} out of the polar stratosphere rather than a twofold increase in stratospheric inventory. Furthermore, Hagemann *et al.* (*6*) admit that their estimate of total bomb-derived radiocarbon produced up to the testing moratorium is only a rough figure. They obtained it by scaling up the January 1957 measured inventory according to the total number of megatons exploded, allowing as well for a difference in the proportion of tests conducted in the air and at surface level.

Clearly, then, there is considerable uncertainty in the stratospheric inventory of July 1959. As a result, only a range is given in Table 4. The upper limit, 17×10^{27}, is the value discussed above, while the lower limit is taken as the measured inventory in 1957—approximately 8×10^{27} atoms (*6*).

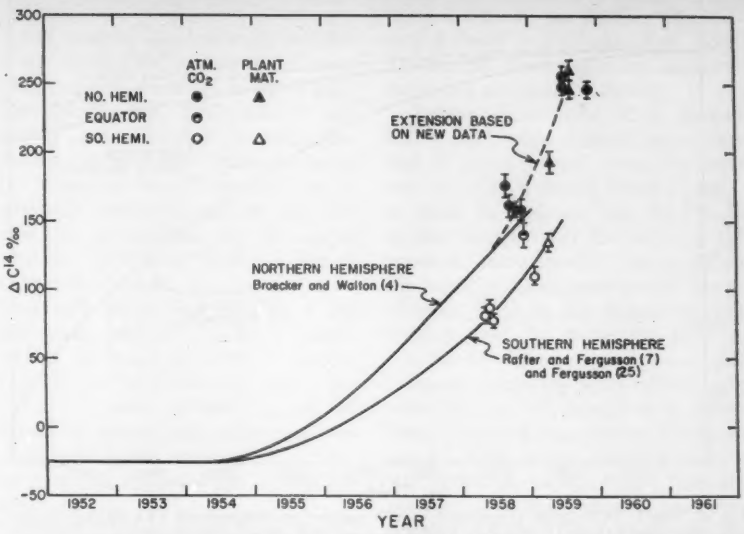

Fig. 1. The C^{14}/C^{12} ratios [expressed as ΔC^{14} values (see *9*)] for the Northern and Southern Hemisphere tropospheres, plotted as a function of time. The solid curves are those published by Broecker and Walton (*4*) and by Fergusson (*7*, *25*). The data on which the extension points and dashed curve are based are given in Table 2.

Future Distribution

About 90 percent of the carbon within the ocean-atmosphere-biosphere system is in the ocean. Consequently, prediction of the future distribution of bomb-produced C^{14} among the various carbon reservoirs depends mainly on having an adequate model of ocean circulation and ocean-atmosphere interaction. The model used here (shown schematically in Fig. 2) is based on estimates of the steady-state C^{14}/C^{12} ratios in the ocean and atmosphere; full details are given in (*15*). The ocean-atmosphere-biosphere system has been divided into ten reservoirs, which are defined in detail in Table 5; each is assumed to be so well mixed internally that its C^{14} concentration is uniform. Transfer of CO_2 between the reservoirs is as indicated by the arrows (Fig. 2), which are numbered to denote the rates of transfer required for steady-state distribution of C^{14} throughout the system (*15*). The mean residence time (in years) of a carbon atom within a given reservoir is indicated by the numbers in parentheses under the reservoir names. Because there is some uncertainty about the number and mean residence time of carbon atoms in the humus of terrestrial soils, this reservoir has not been included in the model. As shown be-

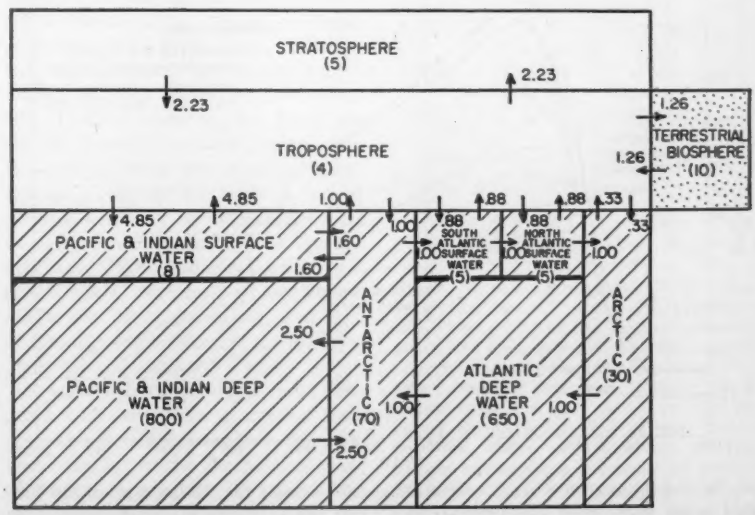

Fig. 2. Circulation model showing the tenfold division of the dynamic carbon cycle. Arrows indicate modes of mixing, and the numbers associated with each arrow give the rate of transfer of CO_2 (in gram molecules/yr $\times 10^{-18}$). The numbers in parentheses are the mean residence times of CO_2 molecules in the reservoirs (see *15* for details).

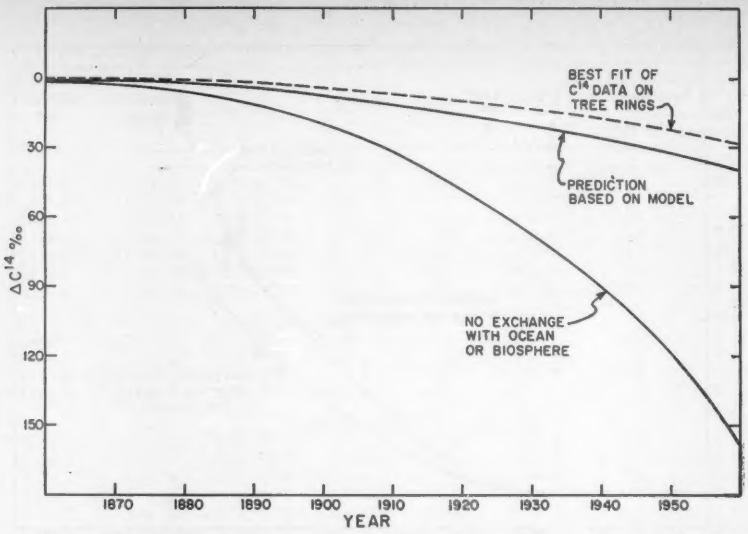

Fig. 3. The C^{14}/C^{12} ratios (expressed as ΔC^{14} values) in atmospheric CO_2 plotted against time; the plot is based on theory (solid curves) and on measurements (dotted curve). The decrease in ΔC^{14} is due to the addition to the atmosphere of C^{14}-free CO_2 produced by the combustion of coal and oil. The curve based on measurements is primarily from the work of Fergusson (*26*).

low, there is evidence that the mean residence time of carbon atoms in humus is measured in hundreds rather than tens of years; thus it is of little present importance in connection with the problem of bomb-produced C^{14}.

The stratospheric residence time given in Fig. 2 is 5 years, as determined by Hagemann *et al.* (*6*) on the basis of C^{14} data. From consideration of Sr^{90} inventories, Feely (*17*) has suggested that the residence time may be closer to 1.5 years. It is possible, however, that Sr^{90} is transferred to the troposphere faster than C^{14} because Sr^{90} becomes attached to particles sufficiently large to settle, whereas C^{14} remains as a gas. In addition, the stratospheric residence time may depend on altitude and latitude of injection—a longer residence time for low latitudes and high elevations.

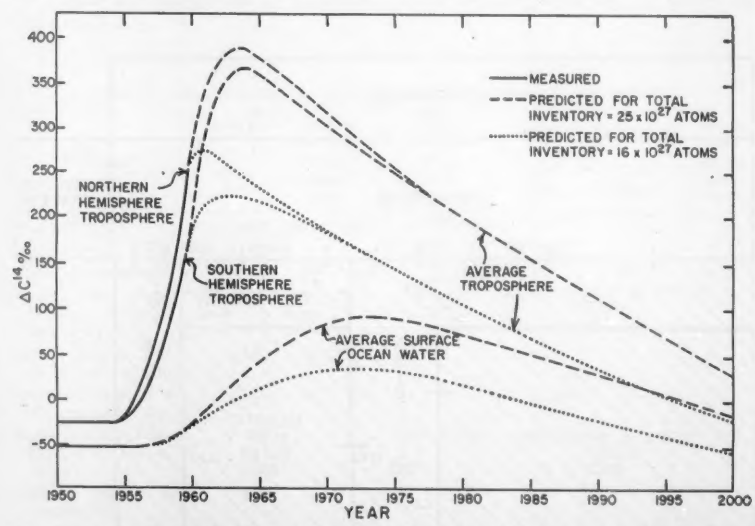

Fig. 4. Predicted C^{14}/C^{12} ratios (expressed as ΔC^{14} values) for the troposphere and surface ocean water from A.D. 1960 to 2000. Two sets of curves are given in order to reflect the limits of uncertainty in the stratospheric inventory of bomb-produced C^{14}. Estimates include the projected combustion-produced CO_2 effect. Direct measurements are available up to 1 July 1959 for the troposphere and through 1957 for surface ocean water. The dates (abscissa) refer to 1 January of the year in question.

In order to check whether the ocean model adequately represents mixing within the dynamic reservoir, predictions based on it have been made, for the period 1850 to 1955, of the C^{14}/C^{12} ratio of atmospheric CO_2 resulting from man's release of C^{14}-free CO_2 by burning coal and oil. The amounts of such CO_2 added to the tropospheric reservoir each decade since 1860 have been summarized by Revelle and Suess (*18*). Calculations in which their data are applied to the model suggested here give the upper solid curve in Fig. 3. For comparison, a dotted curve based on the available analyses of tree-ring samples is plotted on the same graph. When corrected for radioactive decay between growth and measurement and normalized to a common C^{13}/C^{12} ratio (normalization corrects for isotope fractionation), tree-ring data should provide a basis for estimating the C^{14}/C^{12} ratio in the atmosphere at the time of ring growth. Also plotted in Fig. 3 is the C^{14}/C^{12} ratio the atmosphere would have had if there had been no communication between atmospheric CO_2 and bicarbonate dissolved in the ocean or between atmospheric CO_2 and organic matter in the terrestrial biosphere. The theoretical model is seen to provide a fairly good fit to the data, predicting a slightly greater effect of combustion-produced CO_2 than is actually observed. Thus, it is reasonable to conclude that the model provides a fair approximation to the true transfer rates between the atmosphere and other reservoirs.

With the model proposed here, it is possible to predict future distributions of bomb-produced C^{14} throughout the dynamic carbon reservoir. Table 4 gives such results; one set is based on the upper limit of the stratospheric inventory and one on the lower. These predictions are made on the assumption that there will be no further increase in inventories of C^{14} from bomb testing. The total inventory decreases with time as the result of radioactive decay (1 percent per 80 years). On the other hand, the inventory of cosmic-ray-produced C^{14} (given for comparison in column 10) should remain essentially constant.

Inventory values such as those given in Table 4 can readily be converted to ΔC^{14} values in order to show per-millage changes from pre-bomb levels. This con-

version has been made for the tropospheres of the two hemispheres as well as for surface ocean water (the upper 100 meters); the results are plotted in Fig. 4 to A.D. 2000. The solid portions of the curves are fixed by measurements, while the dashed portions are based on the model. The dashed curves also take into account the effect of further combustion of fossil fuels; consumption estimates given by Revelle and Suess (*18*) were used. The predicted reductions in the atmospheric C^{14}/C^{12} ratio resulting from future increments of combustion-produced CO_2 are 65 per mill in 1975 and 140 per mill in 2000. The comparable estimates for the surface ocean are, respectively, 40 and 95 per mill.

The results given in Fig. 4 suggest that the atmosphere will reach its peak C^{14}/C^{12} level before 1963 and that the surface ocean will reach its peak between 1970 and 1975. The ratio for the atmosphere will have dropped to one-half the peak value between 1975 and 1980, and the ratio for the surface ocean will have dropped proportionately by about 1990. In about 50 years the C^{14}/C^{12} level in the atmosphere will have returned to the pre-bomb level. From this time on, the fossil-fuel effect will dominate. All of these conclusions are based on an assumption that there will be no further additions of C^{14} from bomb detonations and on an assumed stratospheric mean residence time of 5 years.

Just how valid the model is will be revealed by continued monitoring of atmospheric and oceanic C^{14}/C^{12} ratios. As discussed below, future monitoring of bomb-produced C^{14} throughout the ocean-atmosphere-biosphere system will also provide considerable information regarding residence times and modes of mixing. If the present model is not adequate, the information that shows its inadequacy will aid in developing a better model. Measurements over the next year or two should resolve the uncertainty in the total inventory.

Potential Tracer Applications of Bomb-Produced Carbon-14

The radiocarbon produced by nuclear tests provides a potentially useful tracer in the study of a number of problems in the natural sciences. In order to suggest the potentialities of this tracer to scientists in various fields, we cite four examples of its use below. These examples involve the fields of soil science, biochemistry, limnology, and oceanography.

Soil science. An important variable in the field of soil science is the mean life of organic materials in active soils. Theoretically, the C^{14}/C^{12} ratio in soil carbon is an indicator of mean life. Prior to the addition of bomb-produced C^{14}, however, the C^{14} method was a relatively insensitive tool for this purpose, as is indicated by the curves marked "1900" and "1950" in Fig. 5. In pre-bomb days, soil organic fractions with mean lives between 4 and 256 years had C^{14}/C^{12} ratios almost within the range of error of precise radiocarbon measurements (±5 per mill). To some extent this lack of sensitivity stems from the post-1900 decrease in the atmospheric C^{14}/C^{12} ratio, which depended on combustion-produced CO_2.

Figure 5 also shows how organic materials of soils will react to the addition of bomb-produced C^{14}. By 1964 it should be possible to measure residence times with an accuracy of better than 10 percent. Since active soils contain a mixture of organic materials which undoubtedly have different residence times, interpretation of soil data will no doubt be a complex problem. This will be especially true if there exists a chemical decay sequence of organic fractions such that one fraction is derived from the next. For example, if *A* decays to *B*, *B* to *C*, *C* to *D*, and so on, then *D* might have a very short residence time in the soil and still not show any bomb-produced C^{14} for many years. Fractions *A*, *B*, and *C* would act as buffers in the passage of bomb-produced C^{14} to fraction *D*. Nevertheless, measurements carried out over a number of years on various mechanical and chemical fractions of a given soil should provide valuable information. Provided no more above-ground bomb testing is carried out, the period of greatest sensitivity for organic fractions of short residence time will be between now and 1965. Sometime within this period the C^{14}/C^{12} ratio of the atmosphere will begin decreasing, and thus additional complications in the interpretation of soil data will enter.

Since a knowledge of the mean residence time of carbon in organic fractions in soil is necessary to a complete understanding of the geochemical cycle of carbon, a program of radiocarbon measurements on such fractions has been initiated at the Lamont Observatory. To date, five measurements have been made, all on fractions of a soil sample taken in early June 1959 in a wooded area on the Lamont Observatory grounds in Palisades, New York. The sample descriptions and measurement data are given in Table 6.

Carbon-dioxide given off by the soil had a ΔC^{14} value slightly lower than that of plants formed during the summer of 1958—a finding which suggests, as might be expected, that this CO_2 is largely derived from the decay of the previous year's growth. The result on the humic acids suggests a mean life of between 50 and 250 years for this fraction (see the curve for 1958 in Fig. 5). On the other hand, the organic fractions remaining after humic acid extraction are old enough to be measured by natural radiocarbon, having an age of 2000 years (equivalent to a ΔC^{14} value of −229). Aside from the escaping CO_2, the coarse organic materials from the 21-inch composite are the only fraction which might have taken on bomb-produced C^{14}; this fraction probably has a mean residence time of 50 years or less. However, the fine organic materials from the same composite show an appreciable age of about 1100 years ($\Delta C^{14} = -132$). The

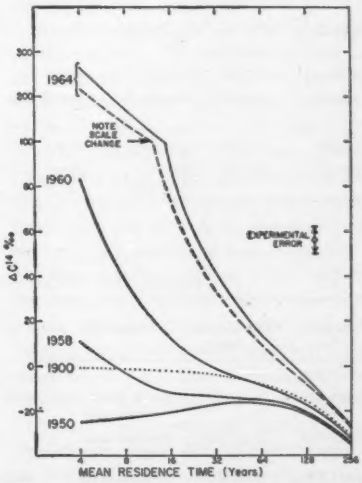

Fig. 5. A plot showing the C^{14}/C^{12} ratio (expressed as ΔC^{14} along the ordinate) which one would find in Northern Hemisphere soil organic material if the organic material had a mean residence time of x (the abscissa) and were collected and measured in the year M. Two sets of curves are given for 1964 in order to reflect the limits of uncertainty in the present stratospheric inventory of bomb-produced C^{14}. Curves are given for M = 1900, 1950, 1958, 1960, and 1964. The curves are based on the model of the carbon cycle described in this article. The dates refer to 1 January of the year in question.

Table 6. Radiocarbon analyses on various fractions of a soil sample from a wooded area in Palisades, N.Y., collected in the late spring of 1959.

Material	Separation procedure	Carbon (gm per kg of soil*)	δC^{14}	δC^{13}	ΔC^{14}	Sample No.
CO_2 given off by soil	Absorption in KOH located under barrel inverted over soil	†	107 ± 7	−23.2	108 ± 7	L528-D
Humic acids from surface soil (upper 6 in.)	NaOH soluble portion of fine fraction (<40 mesh)	5 ± 2	−25 ± 7	−24.2	−28 ± 7	L528-G
Residual organic fraction in surface soil after removal of humic acids	NaOH insoluble portion mesh of fine fraction (<40)	8 ± 2	−218 ± 9	−24.7	−229 ± 9	L528-P
Coarse 100-percent organic fraction of bulk soil (composite from 0 to 21 in. depth)	Stayed on screen (ordinary window screen) when soil was washed through	4 ± 2	−1 ± 7	−24.4	−2 ± 7	L528-K
Fine fraction of bulk soil (composite from 0 to 21 in. depth)	Went through window screen (includes fine organic)	10 ± 2	−123 ± 9	−23.0	−132 ± 9	L528-L

* Weight of soil is based on a depth of 21 in. Since most organic matter is in the upper third of this depth, values in column 3 would be tripled if only 7 in. depth were taken. † At the observed rate of escape of CO_2 from the area under the barrel (that is, 7 gram molecules/m^2 month), about 1.2 percent of the total soil organic carbon was being converted to CO_2 in 1 month.

Table 7. Data relating to the time lag in C^{14}/C^{12} ratio between human blood and the atmosphere.

Sample No.	Collection: Date	Collection: Location	Material	δC^{14} (‰)	δC^{13} (‰)	ΔC^{14} (‰)	Date of equivalent in atmosphere	Time lag (yr)
			Northern Hemisphere					
L505-A (*20*)	1/1/59	Palisades, N.Y.	Breath CO_2	106 ± 8	−21.8	105 ± 8	1/58	1.0
L505-B (*20*)	1/1/59	Palisades, N.Y.	Blood	115 ± 8	−16.5	102 ± 8	1/58	1.0
L516-A	3/4/59	Carson City, Nev.	Blood	118 ± 7	−16.4	104 ± 7	1/58	1.2
L516-D	6/1/59	Carson City, Nev.	Blood	135 ± 6	−18.0	126 ± 6	4/58	1.1
L516-G	12/17/59	Carson City, Nev.	Blood	183 ± 6	−17.8	175 ± 6	12/58	1.0
L569-A	1/19/60	Palisades, N.Y.	Blood	194 ± 6	−16.7	184 ± 6	1/59	1.0
L569-B	1/13/60	Palisades, N.Y.	Breath CO_2	213 ± 6	−18.2	207 ± 6	2/59	1.1
			Southern Hemisphere					
L519-A	3/6/59	Lima, Peru	Blood	70 ± 6	−17.2	56 ± 6	11/57	1.4
L516-C	3/15/59	Ushuaia, Argentina	Blood	33 ± 8	−19.2	20 ± 8	11/56	2.4

Table 8. Effect of bomb-produced C^{14} in rivers and lakes.

Sample No.	Collection date	Material	δC^{14}	δC^{13}	ΔC^{14}	Annual increase (‰)	Fraction of atmospheric increase
		Truckee River, Calif.					
L415-NN (*21*)	Aug. 1957	Plants living under water	−40 ± 7	−18.8	−53 ± 7		
L415-OO (*21*)	Aug. 1957	Mollusk shell	6 ± 7	−4.7	−34 ± 7		
L415-PP (*21*)	Aug. 1957	Mollusk meat	−23 ± 7	−27.0	−20 ± 7		
L487-H	Aug. 1958	Dissolved HCO_3^-	−1 ± 7	−8.5	−34 ± 7	< 20	< 0.40
L487-F	Aug. 1958	Mollusk shell	−8 ± 6	−3.1	−52 ± 6		
L487-G	Aug. 1958	Mollusk meat	−22 ± 6	−20.3	−32 ± 6		
		Weber River, Utah					
L415-S	Aug. 1957	Plants living under water	−146 ± 6	−32.2	−141 ± 6	17 ± 11	0.34 ± 0.22
L487-O	Sept. 1958	Plants living under water	−116 ± 9	−23.7	−124 ± 9		
		Great Salt Lake, Utah					
L415-FFF (*21*)	June 1958	Brine shrimp	−111 ± 7	−16.3	−132 ± 7		
L415-GGG	June 1958	Dissolved HCO_3^-	−93 ± 6	+3.8	−150 ± 6	67 ± 12	0.64 ± 0.12
L542	Aug. 1959	Brine shrimp	−39 ± 9	−15.0	−60 ± 9		

fact that separation into fractions of different age is possible by straight sieving is consistent with the prediction that comminution of organic litter is time-dependent. Just how far this method of separation by age can be carried is not known from the data now available. An interesting and consistent fact to point out is that a recombination of the humic acids with the residual organic fractions gives an organic mixture with almost the same ΔC^{14} value as that measured for the fine fraction from the soil composite.

Along the same line, Tamm and Ostlund (*19*) in Sweden have recently published radiocarbon measurements on total organic matter found in three horizons of a soil profile. They find a progressive decrease in radiocarbon with depth, total organic matter in the deepest layer having a mean residence time of about 400 years. Further analyses of organic materials from soils collected over the next few years will make it possible to draw firmer conclusions. The data reported here merely illustrate the type of information which can be obtained.

Biochemistry. Broecker *et al.* (*20*) pointed out the value of bomb-produced C^{14} as a tracer in determining the mean life of carbon in various tissues of animals and men. Just as Fig. 5 gives quantitative data for soils, so Fig. 6 gives information on residence time for human tissue. Fig. 6 gives data only for relatively short residence times and differs from Fig. 5 in that blood, rather than atmospheric CO_2, is taken to be the source of the carbon in human tissue. Where data from direct analyses of the blood are not available, it is assumed that the ΔC^{14} value for blood is the same as that for atmospheric CO_2 one year earlier. This assumed one-year lag is based on the data given in Table 7 for three residents of the United States (five samples). To be sure, the actual lag depends on such factors as food storage time and the relative proportions of terrestrial and aquatic foods in the diet. No doubt dietary differences account for the high lag times for the Southern Hemisphere listed in Table 7. In any given case, however, an analysis of the blood will indicate the actual lag time. Without such an analysis, it is probably safe to assume a lag time of one year if the person in question ate what might be called a typical American diet.

To date, only one sample of human tissue has been analyzed. Perry Hudson provided us with a composite sample of

blood-free muscle tissue taken during an autopsy of a New York City resident who died in January 1960. The δC^{13} and ΔC^{14} results are, respectively, -16.2 and $+ 143 \pm 10$ per mill. On the basis of the one-year lag time suggested above, the mean residence time of carbon atoms in muscle tissue is 0.8 ± 0.2 year. Again, it is well to emphasize that studies such as this should be carried out while the atmospheric curve is still rising, a situation which even now may be coming to an end.

Limnology. An understanding of the CO_2 cycle in fresh-water systems is important in a number of scientific problems, such as metabolism studies in lakes, studies of probable sources of ground and thermal waters, and radiocarbon dating of materials formed in fresh water. One parameter that is necessary in many of these studies is the invasion rate of CO_2 from the atmosphere into the body of water. Broecker and Walton (*21*) estimated such invasion rates for several closed lakes in the Great Basin, using the steady-state C^{14} concentrations in these lakes and in the rivers which supply them. Another way to estimate these invasion rates is to observe the rate of build-up of bomb-produced C^{14} in the lakes. Because of its low bicarbonate concentration and its large ratio of surface area to volume, Great Salt Lake should show a far more rapid increase in C^{14}/C^{12} ratio than the other lakes studied. Therefore, it is the best lake to sample periodically in order to provide data for accurately estimating the invasion rate of CO_2. Measurements spaced 14 months apart [June 1958 to August 1959 (see Table 8)] show that ΔC^{14} values for Great Salt Lake rose 67 ± 12 per mill during this period. This rise can be used to predict the invasion rate in the following manner.

Let us assume that the total CO_2 concentration in the lake is at steady-state and that the annual water input is equal to the water lost by evaporation. The following equation will then express the rate of change in the C^{14} concentration of the dissolved bicarbonate in the lake:

$$\frac{d\Delta C^{14}_L}{dt} = \frac{l_E k_R}{\bar{h}\, k_L}\left(\Delta C^{14}_R - \Delta C^{14}_L\right) + \frac{R}{k_L \bar{h}}\left(\Delta C^{14}_A - \Delta C^{14}_L\right) \quad (1)$$

where ΔC^{14}_L, ΔC^{14}_R, and ΔC^{14}_A are, respectively, measures of the C^{14} concentration in the dissolved bicarbonate of the lake, in the dissolved bicarbonate of average river water entering the lake, and in CO_2 from the atmosphere over the lake; l_E is the mean annual linear evaporation rate; $\bar{h}$ is the mean depth of the lake; and k_L and k_R are, respectively, the total CO_2 concentration in the lake and in average river water entering the lake. R is the rate of invasion of CO_2 from the atmosphere into the lake.

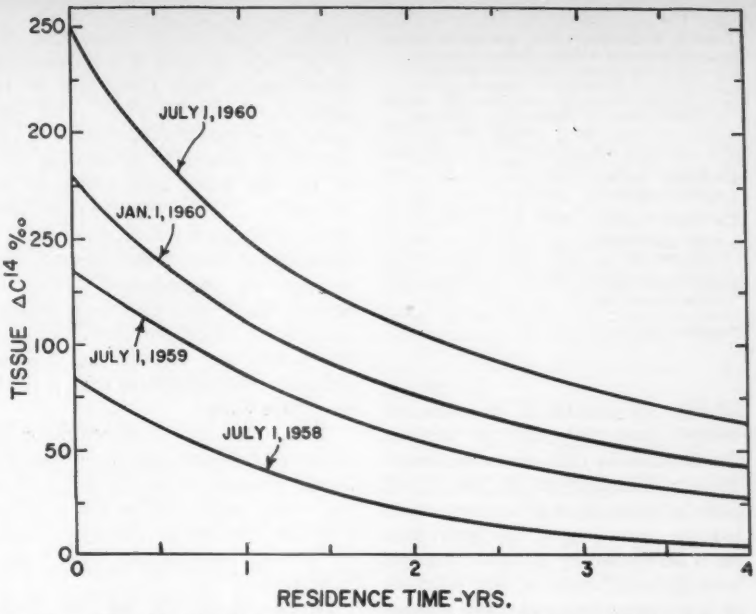

Fig. 6. A plot showing the C^{14}/C^{12} ratio (expressed as ΔC^{14} along the ordinate) in body tissue as a function of mean residence time (abscissa). A 1-year time lag between blood and atmospheric CO_2 is assumed. The date on each curve represents the date of collection of a tissue sample.

Mean ΔC^{14} values based on actual measurements are $+190$ for ΔC^{14}_A and -100 for ΔC^{14}_L. Since the rivers draining into Salt Lake are as yet only slightly influenced by bomb-produced C^{14} (as discussed below), the mean value assumed for ΔC^{14}_R is the one measured by Broecker and Walton for the Bear River at Preston, Idaho (*21*)—that is, -255. The values of l_E, $\bar{h}$, k_L, and k_R are, respectively, 1.1 meter per year, 3.6 meters, 6.2 gram molecules per cubic meter, and 2.8 gram molecules per cubic meter (*21*). The only variables in Eq. 1 which are as yet unspecified are $d\Delta C^{14}_L/dt$ and R. The former can be calculated from the 67 per mill per year rise given above. Solution for the remaining variable R gives an invasion rate of 6 gram molecules per square meter per year. When all the uncertainties are considered, it is probable that the actual value is not less than 3 nor greater than 8 gram molecules. This agrees well with the best estimate of 4 gram molecules per square meter per year obtained with the steady-state assumptions (*21*).

Bomb-produced C^{14} is also a means of determining the dominant source of bicarbonate found in fresh waters. In most cases there are three sources: (i) sedimentary carbonates which are free of radiocarbon, (ii) atmospheric CO_2, and (iii) CO_2 produced in the decay of organic materials either in the drainage-basin soils or in the sediments underlying the body of water. As shown by Munnich and Vogel (*22*) and by Broecker and Walton (*21*), measurements of *natural* C^{14} can indicate the relative contributions of source (i) and of sources (ii) and (iii) combined. However, the contributions of sources (ii) and (iii) were not separable by this means, since concentrations of C^{14} in soil CO_2 and in atmospheric CO_2 were very nearly the same prior to bomb testing. With the addition of bomb-produced C^{14} to the atmosphere, soil CO_2 now lags considerably behind atmospheric CO_2 in C^{14}/C^{12} ratio; hence, under certain circumstances a differentiation between sources (ii) and (iii) may be made on this basis.

A third area of application is that of determining the transit time of ground water. Suppose that the relative contributions of the three sources of dissolved bicarbonate are known,

Table 9. Radiocarbon data bearing on the replacement rate of Cariaco Trench water.

Water mass	Date	No. of samples	Av. ΔC^{14} (‰)	S.D.* (‰)
Caribbean surface water (measured)	1955	8	−57	±9
Caribbean surface water (estimated from model)	1890		−43	
Cariaco trench deep water (measured)	1957	2	−40	±7

*Standard deviation.

through an analysis of the type described above. Suppose, too, that the C^{14}/C^{12} ratios in each can be estimated. Then a measurement of the C^{14}/C^{12} ratio in a sample of ground water will indicate how long it has been since the water picked up its CO_2. It should be emphasized, however, that estimates of both travel time and source cannot be made from the same C^{14} measurement. Hence, periodic C^{14} measurements should be made and combined with other information, such as stable carbon isotope ratios and tritium measurements.

Other examples of the use of C^{14} data from fresh-water systems are measurements of the Truckee River at Truckee, California, and of the Weber River at Ogden, Utah, which show that the C^{14}/C^{12} ratio is rising at a considerably lower rate in these rivers than in the atmosphere (see Table 8). This suggests either that the atmosphere is not the major source of C^{14}-bearing CO_2 or that the water, after taking on atmospheric CO_2, resides more than three years in the ground-water system before reaching the river. If the transport time through the ground-water system is actually short, then CO_2 from organic decay is the major source of C^{14}-bearing bicarbonate and the decaying organic materials must have mean lives of more than three years.

Thus, while the C^{14} concentration in the atmosphere continues to rise rapidly, limnic processes which involve carbon and which occur over periods of from 1 to 100 years can be effectively studied by periodic measurements of C^{14} concentrations.

Oceanography. Of all the fields mentioned above, perhaps oceanography should benefit most from the study of bomb-produced C^{14}. Already natural radiocarbon has provided much valuable information concerning the turnover time for deep-water masses which exhibit residence times of the order of hundreds of years (*7, 15*). But for studying near-surface masses which communicate with the surface on a time scale of tens of years, natural radiocarbon is of little use. It is in the study of such water masses, however, that bomb-produced C^{14} is most useful.

Consider, for example, the Cariaco Trench in the southern Caribbean, off Venezuela. The circulation times in this trench are of particular interest because it is stagnant in the sense that dissolved oxygen is absent while H_2S is present. The C^{14} measurements for various water samples from the Cariaco Trench are listed in Table 9. They show that the deep waters of the trench have as high a C^{14}/C^{12} ratio as the surface waters supplying the trench. This suggests that mixing occurs in tens rather than in hundreds of years. With the atmospheric C^{14} concentration rising so rapidly, however, the deep and surface waters will begin to show measurable differences unless the mixing occurs in as short a period as several months. The situation is graphically represented in Fig. 7, where the radiocarbon concentrations predicted for the deep waters of the trench are shown for various mean residence times (times of residence prior to mixing with the surface waters of the Caribbean). It is obvious that in 1956 (a representative pre-bomb period) residence times of less than 100 years could not be determined from radiocarbon concentrations. By 1970, however, as shown in Fig. 7 (far right), radiocarbon concentrations will indicate residence times in the range from 0 to 100 years rather effectively. In addition to indicating the mean residence time in the trench, C^{14} data should also make it possible to determine the mode of injection and the rate of mixing of new water added to the trench. For the minimum C^{14} inventory, curves similar to those of Fig. 7 could be drawn. In such a figure each point would be only two-thirds as far above the pre-bomb base level as the corresponding point in Fig. 7.

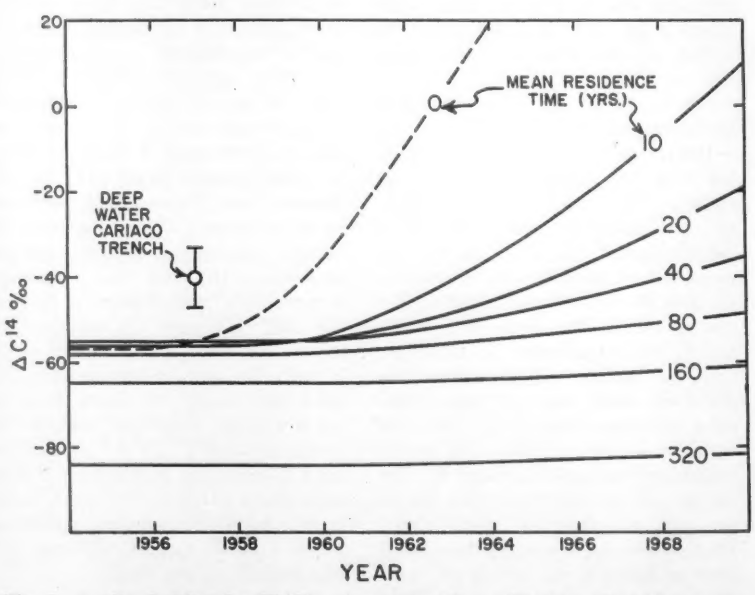

Fig. 7. A plot showing the C^{14}/C^{12} ratio (expressed as ΔC^{14} along the ordinate) as a function of time for various deep-water residence times (T) in the Cariaco Trench. T is defined with respect to the mixing with Caribbean surface water. The curves are based on the model of the carbon cycle described in this article and on an upper limit for the stratospheric inventory of bomb-produced C^{14}. The average of two measurements on deep-water samples from the Cariaco Trench is shown.

Conclusions

Radiocarbon measurements in simultaneously collected tree-leaf and atmospheric CO_2 samples show that the concentrations are often the same but that situations arise where tree leaves give a lower value. A possible explanation for this lack of agreement is that the leaves receive part of their CO_2 through the roots of the tree. Consequently, it appears that direct collec-

tion of CO_2 from the air is the most reliable way to monitor the atmospheric C^{14}/C^{12} ratio.

Further monitoring of tropospheric C^{14}/C^{12} ratios in both hemispheres indicates that the rate of rise roughly doubled during the period from 1 July 1958 to 1 July 1959. This increase may be due either to a doubling of the stratospheric inventory from about 8×10^{27} to around 17×10^{27} C^{14} atoms or to quite rapid fallout of debris added to the polar stratosphere.

Estimates of the future distribution of bomb-produced C^{14} throughout the atmosphere-biosphere-ocean system have been made; for this purpose a mixing model based on steady-state concentrations of natural radiocarbon was used. The predicted inventories are given in Table 4. From these inventories, combined with estimates of the future effects of combustion-produced CO_2, the C^{14}/C^{12} ratios for the atmosphere and for surface ocean water can be predicted (see Fig. 4). The ratio for the atmosphere of the Northern Hemisphere will rise before 1963 to a maximum of from 1.3 to 1.4 times the pre-bomb value, whereas for surface ocean water the maximum will be between 1.09 and 1.15 times the pre-bomb value and will occur between 1970 and 1975. By 1980 the ratio for the atmosphere will have dropped halfway back to the pre-bomb level, while the ratio for the surface ocean will drop proportionately by about 1990. Early in the next century the combustion-produced CO_2 effect will be dominant in the atmosphere, and the C^{14}/C^{12} ratio will have dropped below the pre-bomb value. As illustrated by several examples, bomb-produced C^{14} can serve as a potential tracer in the fields of soil science, biochemistry, limnology, and oceanography.

Due to continuous spiking of the troposphere with bomb-produced radiocarbon, changes in C^{14} concentration throughout the carbon cycle are now occurring faster, by several orders of magnitude, than changes due solely to radioactive decay. Consequently, processes carried out in from 1 year to 100 years are now measurable, or will be so in the next few years. It is hoped that the examples discussed in this article will stimulate the imagination of scientists in many fields, so that the many potential applications of this world-wide tracer may be realized (*23*).

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Brenton Reid Lutz, Vascular Physiologist

Brenton Reid Lutz was born in Woodlawn, Nova Scotia, on 2 June 1890. He graduated from Melrose High School, Melrose, Massachusetts, in 1909. He attended the College of Liberal Arts and the Graduate School of Boston University, earning the degrees of S.B., A.M., and Ph.D. (1917) in physiology. During his early professional years he was awarded the Jacob Sleeper fellowship at Boston University (1914) and a teaching fellowship at Harvard University (1915–1917). In 1918 he married Edna Baldwin. From 1919 to 1939 he held joint appointments at Boston University in the College of Liberal Arts (from instructor to professor of biology in 1927) and in the Medical School (from instructor to associate professor of physiology in 1930). He succeeded Arthur W. Weysse as chairman of the biology department in 1927. He continued to give, annually, a series of lectures in physiology at the Medical School until 1939.

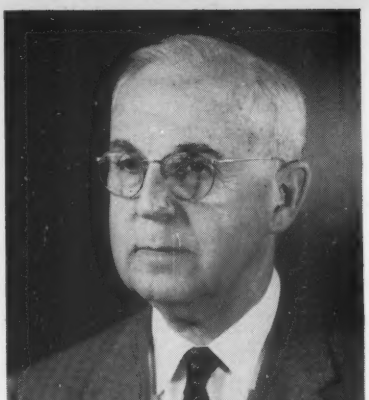
Lutz made blood-pressure measurements in man with Weysse, who introduced the auscultatory method to America in 1913. He worked with Walter B. Cannon in the physiology laboratory at Harvard Medical School. As a result, he developed a life-long interest in the autonomic nervous system and the cardiovascular system, and he became adept in mammalian surgery and physiological instrumentation.

In World War I, Lutz served as a lieutenant in the Sanitary Corps and pioneered in aviation physiology at Mineola, Long Island, under Edward C. Schneider. An important series of

papers was published on the physiological significance of oxygen lack, based on experiments utilizing the rebreather apparatus and the first low-pressure chamber to be used in the United States for pilot training and altitude studies. This research served as the background for the highly organized Altitude Training Program of the Air Force during World War II.

Because of his boyhood association with the sea, Lutz developed an interest in the cardiovascular physiology of marine animals, especially elasmobranchs. He spent several summers at marine stations, including Mt. Desert Island, Bermuda, and Naples. He became an authority on the respiratory and circulatory physiology of fishes. He proposed, with Leland C. Wyman, the evolutionary concept of the carotid sinus mechanism, based on demonstrations of the existence of baroreceptors in the branchial-arch blood vessels and their embryological derivatives in dogfish, *Necturus*, frog, and cat.

For several years he worked under Frederick H. Pratt at Boston University School of Medicine during the development of the all-or-nothing concept for skeletal muscle. Pratt and Marion Reid were using Ranvier's lymph sac preparation, the retrolingual membrane of the frog, for micromanipulative studies of single muscle fibers. Lutz recognized the potential of this preparation for micromanipulative and cinephotomicrographic research on small blood vessels in living subjects. He spent several months in Copenhagen in the laboratory of August Krogh, Nobel laureate renowned for his work on small blood vessels and for his monograph "Anatomy and Physiology of the Capillaries." Upon returning to Boston University, Lutz began an intensive investigation of the neuromotor mechanism of the small blood vessels in the retrolingual membrane. He used microelectrodes to



Brenton Reid Lutz

stimulate the small vasomotor nerves, and he took motion pictures through the microscope to record significant vascular responses. In 1940, Lutz was the producer (with George P. Fulton) of perhaps the first critically edited and fully titled motion-picture film on small blood vessels, designed to be presented as a scientific paper before a professional society. This monochrome silent film, *Control of Small Blood Vessels*, is still in demand as a teaching aid 20 years after its release and in spite of the existence of newer Kodachrome films on related subjects, prepared by his colleagues. During this work he became convinced that Krogh's Rouget-cell concept of capillary contractility is not valid, at least as a general principle, and that capillaries are not contractile in higher vertebrates. He discovered the sphincter-like activity of the smooth muscle cells which occur at the point of origin of small precapillary arterioles from the supplying arteriole. He demonstrated that such "vascular sphincters" may be regulated by the perivascular nerve plexus, since stimulation of nerve components with a microelectrode produced a marked vasodilation.

Lutz collaborated with Fulton, his student, in the use of the cheek pouch of the golden hamster as a living laboratory for microcirculatory studies and for investigations of problems of growth, especially with reference to the role of the vascular system.

He was a member of the American Physiological Society, the American Association for the Advancement of Science, the New York Academy of Science, the American Association of Anatomists, and the American Zoological Society. He was a senior charter member of the Microcirculatory Conference. His honorary awards included election to Phi Beta Kappa, Sigma Xi, and the American Academy of Arts and Sciences. In 1950, he was named first university lecturer at Boston University.

Although he retired in 1957, as emeritus professor, Lutz continued to work in the department of biology as principal investigator on a grant from the National Heart Institute in support of research on the role of the microcirculation in hypertension. The training of graduate students and the premedical preparation of future doctors were outstanding features of his highly productive career. His untimely death interrupted the completion of a paper on comparative vascular patterns, written in collaboration with a college senior working as an honors student on a National Science Foundation Undergraduate Research Participation Program. Lutz built a two-man teaching program in biology into a vigorous department of balanced teaching and research. He will be long remembered by his colleagues and especially by his students, who owe him so very much.

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Science in the News

Nixon on Science: Policy Paper Emphasizes Basic Research; Calls for Special Institutes

The Vice-President issued the second of a series of position papers last week, this one on "The Scientific Revolution." Its major proposal was for the organization of more interdisciplinary research institutes, along the lines of Brookhaven National Laboratory (for research in peaceful uses of atomic energy).

Nixon singled out the recently proposed national meteorological institute as a good example of what he had in mind. He said the field was being inadequately supported (only 14 Ph.D.'s in meteorology were graduated last year), and he pointed out that the heavy investment in equipment would probably be beyond the reach of any one institution. He noted, in particular, the need for a center in which specialists in the various interested disciplines would work closely together to their mutual advantage. The paper summed up the case for the institutes by emphasizing the increasing interdependence of the more specialized fields.

"A logical extension of the nascent merging of different sciences in a common cause," the paper said, "is the mutual use of theory, techniques, and instrumentation which were once the province of a single science. This fusion has created a new dimension in science. Its fulfillment is usually beyond the ability of our conventional research structure."

Nixon said the institutes would not be "controlled, financed, or operated," by the federal government, but that the National Science Foundation should "take the leadership in sponsoring" the institute, which would be "established cooperatively by our universities which engage in graduate research programs" and "governed by boards created by these universities." Nixon said the federal government would contribute funds to match those made available by state and private contributions.

The paper said such institutes would play a growing role in future years, both as research centers and as training grounds for graduate students, but that "they should in no way preempt the role of the university, nor its separate and valid claim for our support." The paper contained no indication of the size of the institute program, of how much federal spending might be involved, or of how actively the federal government might take the lead in encouraging the universities to establish the proposed institutes.

The proposal for more research institutes took up about a third of the 3000-word paper. The remainder contained nothing to startle or offend anyone, other than those who might criticize it for what seemed to be a lack of sense of urgency. Nixon said that science is important, that scientists are important, and that we ought to appreciate this importance, both of science and the men who work in it, more than we have in the past.

He said that, while the Soviet Union is ahead of America in a few limited areas, particularly high thrust rockets, the United States remains clearly ahead of the Soviets in over-all scientific achievement. He said that the Soviet lead in a few areas is a reflection of their ability to use the powers of the state to compel a heavy concentration of their resources in a few areas given high priority. "In contrast," Nixon said, "their efforts in fields such as medicine are relatively mediocre as compared to ours."

Nixon said the public should understand the importance of basic research. He said Americans tend to be impressed by the practical advances of science, but to be unaware of the basic research that makes the advances possible. "Very few of us," said the Vice-President, "have an adequate conception of the endless hours spent by scholars studying the electron, hours without which we would not have our television sets."

Nixon recalled a briefing he had attended at which Herbert York, director of defense research and engineering, talked about what might be learned from space exploration. He said York concluded his exposition by telling his listeners that they could forget all the specific points he had mentioned if they would only remember that the most important thing to be learned from space exploration probably wasn't included in the briefing because no one could now conceive what it might be.

As for recommendations, Nixon began by saying "we must make the necessary education available to those who have the desire and the ability." But there were no specific recommendations on this, perhaps because Nixon has already gone on record as favoring an expanded program of federal scholarships and student loans. At least so far as the paper goes, Nixon does not see any present or imminent shortage of scientific personnel. "Given these creative men and women," he said, "—and we are fortunate we have so many of them—what must we do to aid them to . . . help us meet the challenge." And a little further on, "We have among us the creative men and women to meet the challenge."

Nixon said "we must encourage and back [the scientists] to the utmost." He cited the need for giving scientists "the freedom to explore," for supplying them with adequate facilities ("seismographs, oceanographic ships, astronomical observatories, or whatever is the need"), and seeing that funds were available for "adequately salaried collaborators, assistants, instruments, and supplies."

Background of the Paper

Last month Nixon announced the organization of a group of a dozen key policy advisers in various fields, whose duties are to include the preparation of position papers in their various special fields. Some of these might be used as the basis for speeches, others, considered less suitable for delivery at a political rally, would be issued, like the current paper on science, as policy statements. The principal sources of the science paper were Joseph Kaplan of the UCLA physics department, who served as chairman of American activities during the International Geophysical Year, and John H. Heller, New England Institute for Medical Research. Their version was heavily rewritten by Nixon and by James Shepley of *Time-Life*, who took leave to become chief of staff of Nixon's policy advisers.

The unavoidable political orientation of any statement issued during the

campaign explains such peculiarities as the talk of awarding federal assistance through a system of "matching grants." Matching grants with the spending of state governments (as opposed to outright grants) has become one of the keystones of Republican orthodoxy. But any matching that comes about under such a project as the National Meteorological Institute is likely to involve a very big federal grant to "match" modest contributions from universities and the foundations.

The report of the National Academy of Sciences committee which recommended the establishment of the institute called for a capital investment of $50 million over a 5-year period and an annual operating budget of $15 million. All the money would come from the federal government, although some NSF officials hope that there would eventually be some contributions from private sources.

Similarly, Brookhaven National Laboratory, cited by Nixon as an example of what he has in mind, is supported almost entirely by the federal government. Brookhaven cost the government $30 million last year in direct support.

A few researchers and a small amount of equipment were financed by the participating universities, but even here the actual money often came from a grant the government had given to the university to support the scholar or to buy the equipment.

Contributions from state governments and private industry, as a practical matter, are not likely to be important. Even on programs like slum clearance and road building, where the benefits to the state are very direct, the federal government often contributes $4 or more to every dollar put up by the state or local governments. In the case of the research institutes, where the benefits are clearly to the nation, rather than to the state in which the institute might be located, no one really expects more than token participation by state and local governments or by private business. Thus, the institute program will almost certainly depend on how willing a Nixon administration would be to go beyond the terms of the paper, which specifically says the government will not finance the institutes. It is difficult to find anyone who believes that Brookhaven would exist or that the proposed institute for meteorology is likely to come into existence unless the federal government is willing to put up the money.—H.M.

News Notes

Radiation Limits Reduced to One-Third Present Levels by Atomic Energy Commission

The Atomic Energy Commission has established sharply lower limits for radiation exposure by amending its regulation for the protection of employees in atomic energy industries. Therefore, the radiation limit for members of the general public beyond the confines of atomic plants is also radically changed, for the rule is that the public may not receive more than 10 percent of the maximum exposure permitted radiation workers.

The new restrictions, which go into effect on 1 January 1961, will reduce the allowable life-time accumulated dose of radiation received by workers to approximately one-third the amount allowed under the regulation as it now stands. The total external radiation exposure that any worker may accumulate beyond the age of 18 will be lowered to an average of 5 rem per year and to not more than 3 rem in any one quarter. Present limits for radiation workers are 0.3 rem per week (or approximately 15 rem a year), without further restrictions as to accumulated dose.

[A rem (*r*oentgen *e*quivalent *m*an) is a radiation dose of any ionizing radiation estimated to produce a biological effect equivalent to that produced by 1 roentgen of x-rays.]

National Committee Proposes Limits

The radiation exposure levels now in effect are based on the 1957 recommendations of the National Committee on Radiation Protection, a group of radiation specialists who work for government agencies, including the AEC, and for private institutions. The new radiation limits are designed to bring the commission's regulations into accord with the current recommendations of the National Committee.

The committee emphasizes that the lowering of the limits should not be interpreted as indicating that exposures at levels currently permitted by the regulations have caused damage. The NCRP says that the changes, rather, are based on a desire to bring radiation standards into accord with new trends of scientific opinion and to reflect awareness of the probability of a large future increase in radiation uses.

Soviet Scientist Describes Dogs During Space Flight

The reactions of the Soviet dogs Belka and Strelka while they were orbiting the earth in the U.S.S.R.'s satellite spaceship last month were described as follows in the Soviet press by P. Fyodorov, one of the scientists who watched the telecast transmitted from the ship's cabin during flight.

"At the moment of the start the dogs pricked up their ears and looked in bewilderment at the floor of the cabin: What was that unusual noise? During the first seconds of the flight the dogs were worried and tried to rush about. As the ship's speed was accelerated, the dogs were gradually pressed to the floor by the increasing force of gravity. Strelka tried to resist by pressing firmly with [her] legs and anxiously looked around her. Then the animals stood stock-still. The ship had reached its orbit.

"After the great overload, a condition of weightlessness set in. The dogs found themselves in mid-air in the cabin, their paws and heads limply lowered. At first glance the animals seemed lifeless.

"I will not conceal the fact that we were greatly worried during those minutes. Only the readings of the telemetric system reassured us: The pulse and respiration of our travelers gradually returned to normal. We realized that the animals were simply resting after all the 'excitement' experienced during the take-off and were becoming accustomed to the new and extremely unusual sensations.

"Gradually they started to raise their heads and move their paws. Everything was unusual: It was not so simple to manage their own paws in such strange conditions. Belka even became angry and started to bark; but one gradually becomes accustomed to everything, and the animals became accustomed to the condition of weightlessness. They started to eat. . . .

"There were times during the flight when they again became alarmed, but gradually they began to feel at home."

The television system aboard the satellite is reported to have provided valuable motion-picture films. The recording of the images was synchronized with the recordings of telemetric information, making it possible to compare direct observation of the animals with data on the changes in their physiological functions.

In addition to Belka and Strelka, the spaceship carried 13 white mice, 15 gray mice, and two white rats. These were the first animals ever to be returned from outer space.

The following laboratory specimens were also aboard: insects—*Drosophila* in 15 flasks; plants—spiderworts in two flasks, *Chlorella* in eight ampules in a liquid nutritive medium in suspension form and in four ampules in slant agar; fungal cultures—*Actinomyces* in 14 ampules; seeds—corn and different varieties of wheat, peas, onions, and *Nigella*. The space cabin also contained small segments of skin (human and rabbit) in two ampules; HeLa cancer cells in six ampules; and microbes—KK-12 intestinal bacilli in 11 ampules, B intestinal bacilli in six ampules, intestinal bacilli of the aerogenes type in four ampules, butyric acid fermentation bacteria in two ampules, staphylococci in two ampules, deoxynucleic acid in six ampules, T-2 bacteriophage in three ampules, and 13-21 bacteriophage in three ampules.

Presidential Candidates Urged To Promote Nuclear Test Resumption

Thomas E. Murray, a Democrat and former member of the Atomic Energy Commission, has written an open letter to presidential candidates John F. Kennedy and Richard Nixon urging them to promote the resumption of fallout-free nuclear weapon tests underground and in outer space. Murray, who is now a consultant to the Joint Congressional Committee on Atomic Energy, holds that the test suspension has jeopardized this country's atomic superiority. He says there is a pressing need for the development of weapons suitable for "rational military purposes," emphasizing that he does not mean larger and larger weapons of mass destruction.

He warns that support of test resumption would be "an act of political courage" that would be "unpopular both at home and abroad," yet a "necessary act of political wisdom." At the same time he stresses that the testing issue "must not be exploited for partisan advantage."

Both Kennedy and Nixon have said in recent public statements that the test suspension should be continued as long as there is hope of progress on a permanent test-ban agreement with the Soviet Union.

Scholarship Programs Bring Two Groups of Africans to the U.S.

A program to bring from 100 to 200 African students to this country each year has been announced by a group of 24 colleges and universities. The students will receive all-expense 4-year scholarships from the participating schools. Their governments will pay their travel expenses. A pilot program, involving 23 Nigerian students, has brought 23 students here to begin their studies this fall.

The colleges include: Amherst, Barnard, Brandeis, Brown, Bryn Mawr, Columbia, Cornell, Dartmouth, Harvard, Haverford, the University of Minnesota, Mount Holyoke, Notre Dame, Oberlin, Pembroke, the University of Pennsylvania, Princeton, Radcliffe, Smith, Stanford, Swarthmore, Vassar, Wellesley, and Yale. Other schools are expected to join. Additional financial support will be sought from the government as well as from private foundations.

The program has no connection with the 270 African students who became the center of controversy over whether the State Department or the Joseph P. Kennedy Jr. Foundation should put up the money to fly them to the United States in order to take advantage of scholarships which had been arranged through the African-American Student Foundation. The State Department had turned down a request for the $100,000 needed to fly the students here, but had reversed itself after the Kennedy Foundation had agreed to put up the money.

The State Department later conceded that the reversal had come after intercession by an aide of Vice-President Nixon, and after the Student Foundation had already arranged to finance the students' transportation through the Kennedy Foundation. By this week the controversy had been almost forgotten and the first group of 144 students flew to the United States virtually unnoticed in the press. The remaining 130 students were to arrive later this week and early next week.

Koprowski Protests Exclusive Approval of Sabin Vaccine

Hilary Koprowski, director of the Wistar Institute, who discovered the live-virus poliomyelitis vaccine and developed one of the three types well known in this country, has protested the Public Health Service's recent approval for public use of only the Sabin vaccine. In a letter to PHS Surgeon General Leroy Burney, Koprowski said that "there are no scientific data available which warrant such an exclusive endorsement as you have made."

He pointed out that both the Sabin and Koprowski vaccines had been given to more than 9 million people throughout the world with no ill effects, and commented: "If we are to evaluate the safety of all polio vaccines by their effects on only 223 monkeys, as you have done, both the Sabin and Koprowski vaccines again appear to be eligible for approval because this inconclusive test shows no appreciable difference between the vaccines." The Public Health Service found the Sabin vaccine to be less virulent than Koprowski's and one developed by Herald Cox of Lederle Laboratories.

News Briefs

Support for atomic research. The Joint Congressional Committee on Atomic Energy has issued a report asking for increased research in applications of atomic energy. The report said the government had not been imaginative enough in seeking out and supporting promising new areas for applications research. It was also critical of support for Project Rover, the atomic rocket, which the report said was not being adequately financed.

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French nuclear tests. France's third nuclear weapons test will take place on 15 October in the Sahara Desert some 400 miles south of Algiers. According to an unconfirmed report, an underground explosion will be fired that will trigger a series of similar test blasts.

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Sixty-ton satellite. When he was in Finland recently, Soviet Premier Nikita Khrushchev told a factory worker that the Soviet Union would soon send up a 60-ton space satellite—"a whole train."

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Fast transfer reactions. Molecular biophysicists are becoming increasingly aware of the biological significance of fast transfer processes utilizing elementary particles. To inquire into the possible physiological role of fast transfer reactions in eliciting specific interactions in ordered macromolecular

structures, a series of seminar lectures was held at the Massachusetts Institute of Technology during the spring term of 1960. Abstracts of these lectures and pertinent references have been compiled and are available gratis. Requests should be sent to Professor Francis O. Schmitt, Department of Biology, Massachusetts Institute of Technology, Cambridge 39, Mass.

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NASA consultant. The National Aeronautics and Space Administration has appointed 25 scientists as consultants in five fields of space science: astronomy, ionospheric physics, lunar sciences, particles and fields, and planetary and interplanetary sciences. The scientists will advise the NASA subcommittees which make recommendations to the agency's Space Sciences Steering Committee, the committee that serves as a focal point for space science activities and that selects experiments for satellites and space probes.

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Radiation research. The second International Congress of Radiation Research will be held at Harrogate, Yorkshire, England, 5–11 August 1962. It is sponsored by a committee set up in 1958 at the first congress at Burlington, Vermont, and by the Association for Radiation Research. The program will be concerned with the physical, chemical, biological, and medical effects of radiation, particularly ionizing radiations. Research workers in these fields will be invited to proffer original papers and reports of new experimental work. A brochure will be available April 1961. Information may be obtained from the Secretary-General, Mount Vernon Hospital, Northwood, Middlesex, England.

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Plant science. The University of Colorado College of Pharmacy was host to the First Annual Meeting (Plant Science Seminar) of the American Society of Pharmacognosy from 30 June to 2 July. The president of the society is Varro E. Tyler, Jr., and the secretary is Rolf S. Westby, Eli Lilly and Co., Indianapolis 6, Ind. The society was formed by the pharmacognosists of the United States to formalize and perpetuate the standards and ideals of the Plant Science Seminar and to "promote the growth and development of pharmacognosy, . . . to provide opportunities for presentations of research achievements and to promote the publication of meritorious research." Membership is open to graduate students and workers of other nations.

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Chemical Society building. Government officials, educators, and industrial leaders will join with chemists and chemical engineers from many parts of the country in the dedication on 7 October of the new eight-story national headquarters of the American Chemical Society in Washington. Principal speakers will include Glenn T. Seaborg of the University of California; Charles A. Thomas of the Monsanto Chemical Company, St. Louis; and W. Albert Noyes, Jr., of the University of Rochester.

* * *

Science Youth Month. October will again be National Science Youth Month. As the school year begins, national organizations, including the AAAS, will cooperate with Science Service, Washington, D.C., in a month-long program to stimulate young people's interest in science and technology. Organization of science clubs and science fairs in all parts of the country will be stressed. A calendar of events has been prepared, and information has been assembled for those in local and regional organizations who wish to cooperate.

* * *

International medical journal. *Medicina Universalis*, a new periodical published in six languages, has been established to achieve greater international cooperation in the medical field. Headquarters are in Paris, 71 rue Sainte-Anne. Contributions from physicians and biologists of medical and scientific papers are requested for consideration. Ionel Rapaport, assistant professor in the Psychiatric Institute at the University of Wisconsin, has been appointed editor for the United States. Articles may be addressed to him at 418 N. Randall Ave., Madison, Wis.

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Health statistics center. A National Center for Health Statistics has been established in the Public Health Service. Initially the center will have two divisions: the U.S. National Health Survey, which was transferred to it on 15 August, and the National Office of Vital Statistics, which will become part of the center on 1 October. The center's work will supplement but not supplant the statistical work associated with particular Public Health Service programs. Forrest E. Linder, who has been director of the National Health Survey since its inception in 1956, is acting director of the center pending formal establishment of the position of director.

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New nebula. Radio telescope investigations made by scientists at Cambridge, England, have led to the sighting by astronomers at Mount Palomar Observatory in California of a new nebula farther out in the universe than man has ever seen before. The Mullard Radio-Astronomy Observatory at Cambridge fixed the nebula's position at 5000 million light years away. From this information, it was possible to photograph the nebula with Palomar's 200-inch telescope. The two observatories have been cooperating on the project since the nebula's signals were first noted on the Cambridge radio telescopes last year.

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Paleontological research. The annual meeting of the Paleontological Research Institute, originally scheduled for 1 October, has been postponed until 8 October.

Grants, Fellowships, and Awards

Arctic research. The Arctic Institute of North America invites grant applications from research scientists interested in arctic and subarctic North America and in Antarctica. The aims of the institute's grant-in-aid program, which last year numbered 35 awards ranging in value from $400 to $10,000, are to support the work of experienced field scientists and to introduce competent researchers to polar science. The institute favors no particular scientific disciplines but appraises proposals on the basis of their individual excellence. Grants are usually made for a 1-year period. The institute can assist researchers to obtain the use of laboratory facilities and field stations for many kinds of work.

Further information about the program and application forms are available on request from the Arctic Institute of North America, 3485 University St., Montreal 2, P.Q., or 1530 P St., NW, Washington 5, D.C. Applications for 1961 grants should be submitted *before 1 November.*

College teaching. The Woodrow Wil-

son National Fellowship Foundation has announced the opening of competition for its 1961–62 fellowship awards. The purpose of the Woodrow Wilson program is to combat the critical shortage of qualified college teachers by encouraging outstanding students to enter the college teaching profession.

To accomplish this purpose, the foundation, operating under a $24,500,000 grant from the Ford Foundation, annually offers 1000 first-year graduate fellowships to students interested in college teaching careers. The fellowships, which are worth $1500 plus full tuition and fees and family allowances, may be used at any graduate school in the United States or Canada.

The program is open to college seniors or graduates in the humanities, the social sciences, and the natural sciences. Both men and women are eligible. There is no limit on the age of the candidate or on the number of years he may have been out of college. However, the foundation does not accept applications directly from students; candidates for the award must be nominated by a faculty member, *prior to 31 October*. A brochure containing information about the program may be obtained by writing to Dr. Hans Rosenhaupt, National Director, Woodrow Wilson National Fellowship Foundation, Box 642, Princeton, N.J., or from the Woodrow Wilson representative on any college campus in the United States or Canada.

Graduate study abroad. Two hundred fellowships for graduate study in 13 foreign countries will be offered by foreign governments and universities through the Institute of International Education for the academic year 1961–62. The institute has announced that applications for the fellowships are now available and will be accepted until *1 November*.

The scholarships cover tuition and varying amounts of maintenance in universities in Austria, Canada, Denmark, France, Germany, Iran, Israel, Italy, Mexico, the Netherlands, Poland, Sweden, and Switzerland. Students applying for Italian university awards or Austrian, Danish, French, German, Israeli, Italian, or Netherlands Government awards may apply for a Fulbright travel grant to supplement their scholarships. Two additional awards, offered by an American foundation, are for study in any country in the Far East, in South or Southeast Asia, or in Africa.

General eligibility requirements are United States citizenship, a bachelor's degree or its equivalent before departure, language ability sufficient to carry on the proposed study, and good health. Preference is given to applicants under 35 years of age who have not had extensive foreign experience. While married persons are eligible for most of the awards described above, the stipends are scaled to the needs of single grantees.

For further information and application forms, prospective applicants should write to the Information and Counseling Division, Institute of International Education, 1 E. 67 St., New York 21, N.Y., or to any of the Institute's regional offices.

Medical faculty. Nominations are being accepted for the eighth annual series of Lederle Medical Faculty Awards of the Lederle Laboratories Division of American Cyanamid Company. Awards, in amounts not exceeding $10,000 per year, will be made for a term not exceeding 3 years for the support of young individuals of faculty rank who have demonstrated capacities both as teachers and investigators in departments of anatomy, biochemistry, microbiology, pathology, pharmacology, physiology, and medical genetics, in order to help accelerate their progress as responsible members of their faculty and to encourage them to remain in these disciplines.

Applications will also be considered on behalf of candidates from certain clinical departments which have broad responsibilities for undergraduate medical teaching—namely, internal medicine, pediatrics, and general surgery. These candidates should be experienced physicians who intend to orient their full-time interest and activities primarily toward the teaching of clinical medicine as integrated with the basic medical sciences.

In general, an award will not be made to a candidate with a stable salary provided from a departmental budget if the major purpose of the award is to free funds for new but unspecified individuals. A candidate will receive consideration only if it is clearly evident that the departmental funds will be augmented by the full amount of the award.

Nominations should be submitted to the office of the dean of the medical school and should be endorsed by him and also by the administrative head of the sponsoring department. Only one candidate from any school will be considered in any given year. Nominations for 1961–62 should be sent *before 23 October* to Lederle Medical Faculty Awards, Office of the Secretary, Pearl River, N.Y.

Ophthalmology. The National Council to Combat Blindness, Inc., has announced that the closing date for receipt of completed applications for full-time research fellowships, grants-in-aid, and summer-student fellowships for the 1961–62 period, has been designated as *1 March 1961*.

In general, notification to applicants for full-time research fellowships and grants-in-aid will go forward in July, with 1 August as the commencement date for the projects. Under special circumstances, where earlier notification is essential, the Scientific Advisory Committee may consider applications in advance of the scheduled date. Applicants for student fellowships will be notified in May of the action taken by the Scientific Advisory Committee in order that arrangements may be made with their respective institutions to commence work in early summer. Appropriate forms may be obtained by addressing the Secretary, National Council to Combat Blindness, Inc., 41 W. 57 Street, New York 19, N.Y.

Scientists in the News

Sir Wilfred Le Gros Clark, professor of anatomy at Oxford University, was elected president of the British Association for the Advancement of Science at its recent meeting in Cardiff, Wales. He is widely known for his studies of the thalamus and hypothalamus and of nerve regeneration, and for his work as a paleontologist. His *Fossil Evidence of Human Evolution* was published in Chicago in 1955.

Sir Wilfred succeeds Sir **George Thomson**, professor of physical chemistry at Cambridge University and master of Corpus Christi College, whose father was the famous J. J. Thomson, originator 60 years ago of the concept of electrons that opened the atomic age. In his presidential address, Sir George noted the two aspects of science, one concerned with understanding nature, the other with controlling it.

When the past president, Sir James Gray, thanked Sir George for his ad-

dress, he referred to the unique instance of son following father, after half a century, as president of the British Association, pointing out, in addition, that both father and son were Nobel laureates.

Garbis H. Keulegan, an expert in hydrodynamics and hydraulics at the National Bureau of Standards and an internationally recognized authority on density currents, retired on 31 July after more than 38 years at the bureau. He was one of the original members of the bureau's hydraulics laboratory staff and thus has perhaps contributed more than any other one man to the development of the science of density currents in water. His paper, "Equation of motion for the steady mean flow of water in open channels" stands as a monumental piece of work. He has also pioneered in the development of basic laws relating to wave motion and tides.

Perry R. Stout, chairman of the University of California's department of soils and plant nutrition at Davis and Berkeley and director of the Kearney Foundation of Soil Science, received the Stephen Hales Prize Award of the American Society of Plant Physiologists at its recent national meeting at Oklahoma State University. Stout is known for his work on the micronutrient requirements of plants: minerals such as manganese, zinc, copper, molybdenum, and chlorine. He is the codiscoverer of the last two as essential plant nutrients.

Robbin C. Anderson, professor of chemistry at the University of Texas, will be in Washington during the 1960–61 academic year to work with the National Science Foundation as program director for in-service institutes in NSF's Division of Scientific Personnel and Education.

Howard E. Brown, associate professor of mechanical engineering at Texas, will also serve NSF this year. He has been named assistant program director of the foundation's engineering sciences program.

Thomas J. Macek has been named director of pharmaceutical research and development in the Merck Sharp and Dohme Research Laboratories, West Point, Pa. He succeeds **Paul W. Wilcox**, who resigned recently. Since joining the company in 1942, Macek has been closely associated with the development of synthetic adrenocortical steroid hormones and synthetic vitamin B_{12} and with research on antibiotics.

F. A. Kroger, of the Philips Research Laboratories, Eindhoven, Netherlands, has been named Baker lecturer at Cornell University for the fall term. His lectures on "The Chemistry of Solids" will begin on 4 October and will continue through 10 December.

Erwin R. Biel, professor of meteorology at Rutgers University, will discuss "Bioclimatology and Comparative Studies on Dynamic Climatology" as a Sigma Xi national lecturer at a number of colleges and universities during October.

David K. Caldwell has been appointed curator of marine zoology at the Los Angeles County Museum. For the past 3 years he has been fishery research biologist at the U.S. Fish and Wildlife Service Biological Laboratory in Brunswick, Ga.

S. L. Morrison of the British Medical Research Council's Social Medicine Research Unit, London Hospital, will spend the 1960–61 academic year working in the department of medical statistics at the University of North Carolina.

S. L. Hoyt, metallurgical consultant, has returned from Frankfurt, Germany, where he spent 15 months on a North Atlantic Treaty Organization assignment for the U.S. Department of Defense. He is resuming his consultant's practice at his new residence in Berkeley, Calif. (88 Purdue Rd.).

Theodore Holstein, formerly of the Westinghouse Electric Corporation, has been appointed professor of physics at the University of Pittsburgh. During the past year he served as a visiting professor.

Robert L. Wolke, who will initiate a program of teaching and research in nuclear chemistry and radiochemistry at Pittsburgh, has been appointed associate professor of chemistry. He has been on the faculty of the University of Florida since 1957.

Theodore A. Link, well-known Canadian petroleum geologist, has been awarded the Selwyn G. Blaylock Medal by the Canadian Institute of Mining and Metallurgy "for his distinguished service to Canada in the Field of Geology." He is a past president of the American Association of Petrol Geology and also of the Geological Association of Canada and the Alberta Society of Petrol Geologists.

The following research workers at the National Bureau of Standards, Washington, D.C., have received U.S. Department of Commerce gold medals.

Garbis H. Keulegan, who retired in July from the Fluid Mechanics Section, was cited for "many outstanding contributions to the basic laws of hydrodynamics, particularly for his fundamental work on open channel flow, water waves, and modeling laws underlying the science of density-current flows."

Alvin G. McNish, chief of the Metrology Division, was cited for "outstanding contributions of great importance to the special fields of geomagnetism and ionospheric physics, and to the more general fields of metrology and standardization."

Chester H. Page, chief of the Electricity Division, was cited for "rare and outstanding contributions of major significance . . . in the fields of electronics, ordnance, and physical research and measurement, including highly distinguished authorship."

Harold O. Wyckoff, chief of the X-ray Section, was cited for "outstanding scientific research in the field of radiation shielding and measurements culminating in distinguished authorship of a book on radiation protection."

Herbert P. Broida, technical coordinator of free radicals research, and **Arnold M. Bass**, chief of the Free Radicals Section, were cited jointly for "outstanding scientific and administrative leadership in the direction of the Bureau's Free Radicals Research program."

Charlotte Moore-Sitterly of the Spectroscopy Section was cited for "outstanding scientific research in spectroscopy and astrophysics, for distinguished authorship of the definitive tables of atomic energy levels and spectroscopic multiplets, and for internationally recognized interpretations of the solar spectrum."

Leonardo Testa, chief of the glass blowing shop in the Shops Division, was cited for "his outstanding contributions as a creative glass blower to highly significant scientific and technical programs."

Raymond W. Cunningham, formerly assistant to the director of clinical investigations in the Lederle Laboratories Division of the American Cyanamid Company, Pearl River, N.Y., has been appointed executive secretary of the Pharmacology Training Committee in the Division of General Medical Sciences of the National Institutes of Health. He succeeds **George M. Briggs**, who has accepted a professorship at the University of California, Berkeley.

W. E. Sackston, formerly head of the Plant Pathology Laboratory, Canada Department of Agriculture Research Station, Winnipeg, Manitoba, has accepted the position of professor of plant pathology at Macdonald College of McGill University, Ste. Anne de Bellevue, Quebec.

Harry Alpert, dean of the graduate school and professor of sociology at the University of Oregon, has been named editor of the *American Sociological Review*, official publication of the American Sociological Association.

Harold I. Amory, former chief of radiological service at the Walter Reed Army Hospital, Washington, D.C., has been appointed professor and chairman of the department of radiology at the West Virginia University Medical Center.

W. W. Wainio has been appointed to a 3-year term as chairman of the department of physiology and biochemistry, Rutgers University College of Arts and Sciences, New Brunswick, N.J. He succeeds **J. B. Allison**, who, in addition to being the director of the Bureau of Biological Research, is now director of the University Research Council.

Charles H. Southwick, associate professor of zoology at Ohio University, recently returned from 9 months of field study in northern India. As a Fulbright research fellow at Aligarh Muslim University, Southwick conducted a population survey and behavioral study of rhesus monkeys in the province of Uttar Pradesh.

Henry B. Linford, professor of chemical engineering at Columbia University, has been selected as the 1960 recipient of the Edward Goodrich Acheson Gold Medal and $1000 prize of the Electrochemical Society, Inc. The presentation will be made at the society's banquet, which will be held in conjunction with its fall meeting in Houston, Tex., 9–13 October. The award is given biennially for conspicuous contributions to the advancement of any of the objects, purposes, or activities of the society.

Paul F. Hahn, formerly director of the cancer research laboratory and professor of oncology, Meharry Medical College, Nashville, Tenn., has joined the Division of Radiological Health, U.S. Public Health Service, Washington, D.C.

Cutting B. Favour, director of the department of immunology at the Palo Alto Medical Research Foundation, has been appointed professor and chairman in preventive medicine and associate professor in medicine at Georgetown University.

G. Milton Shy, for the past 7 years clinical director of the National Institute of Neurological Diseases and Blindness, has been appointed to the newly created position of NINDB associate director in charge of intramural research. He will be responsible for developing the clinical and basic research program at the institute's laboratories in Bethesda. In addition, he will advise NINDB director Richard L. Masland on the institute's total program and will represent NINDB on the scientific director's staff of the National Institutes of Health.

Milton F. Pravda, nuclear engineer, has joined the Martin Company's Nuclear Division in Baltimore as chief of system design. Formerly he was manager of reactor design for the U.S. Atomic Energy Commission's natural circulation reactor project at the Knolls Atomic Power Laboratories in Schenectady, N.Y.

Two Texas A. and M. faculty members—**G. M. Watkins**, director of agricultural instruction, and **H. C. Mohr**, associate professor of horticulture—have received the American Society for Horticultural Science's annual Leonard H. Vaughn Award for research in vegetable crops. They were honored for their paper on "The nature of resistance to Southern blight in tomato and the influence of nutrition on its expression."

Recent Deaths

W. G. Bickford, New Orleans, La.; 53; research chemist at the Southern Utilization Research and Development Division of the U.S. Department of Agriculture; well known for his work on the chemistry of vegetable fats and oils; 15 Aug.

Sir **Gordon Gordon-Taylor**, London, England; 82; a leading abdominal surgeon and an authority on the treatment of the abdominal injuries caused by modern warfare; fellow of the British Royal College of Surgeons for more than 50 years; twice held a chair of surgery at Harvard University as a visitor; 3 Sept.

David Greene, New York, N.Y.; 67; retired pediatrician and a former associate professor of pediatric medicine at the Flower and Fifth Avenue Hospital; 1 Sept.

Herbert F. Hinners, Scarsdale, N.Y.; 49; chemical engineer for the Lever Brothers Company; 29 Aug.

William J. Moore, Tom's River, N.J.; 82; professor of experimental engineering at Brooklyn Polytechnic Institute for 35 years; later taught mechanical engineering at the University of North Carolina; 29 Aug.

G. Canby Robinson, Greenport, L.I.; 81; retired professor of medicine, Cornell University Medical School; as director of the New York Hospital–Cornell Medical College Association from 1928 to 1935, played a major role in the establishment and building of the New York Hospital–Cornell Medical Center; during World War II directed the Blood Donor Service of the American Red Cross; dean of Washington University Medical School, 1917–20, and of the Vanderbilt University Medical School, 1920–28; was visiting professor at Peiping (China) Union Medical College in 1935 and served as a lecturer in medicine and preventive medicine at Johns Hopkins University from 1936 to 1946; 31 Aug.

Martin D. Whitaker, Bethlehem, Pa.; 58; president of Lehigh University and a leading nuclear physicist; was serving as acting chairman of the department of physics at New York University in 1942 when he joined the metallurgical laboratory at the University of Chicago to work on the program leading to the development of the A-bomb; in 1943, was named director of the new Clinton Laboratories at Oak Ridge, for which he planned the design and operation; 31 Aug.

Book Reviews

The Presidency. Crisis and regeneration. Herman Finer. University of Chicago Press, Chicago, Ill., 1960. 400 pp. $6.95.

When it was suggested recently (22 July, page 215) in these columns that a serious debate, going to the structure of our American government, seemed imminent, it could hardly have been anticipated that the debate would begin so soon. Before the suggestion was made, this book by Herman Finer must have been at the printers. It is now before us. And it does begin the debate with vigor and comprehension. Formally, by title, it is addressed to the Presidency; but like others who have sought to confine their arguments in this matter, Finer escapes to the other regions of government. Ours is a system of shared powers; and it is impossible to consider one branch without allowing for the impact of the others.

Perhaps it is because this is a presidential year that the Finer attack is made on the executive sector and spreads from there to the others. But this is not the only reason. It is, much more importantly, that the Presidency is the only source of positive policy-making and because this has become a matter of life and death for the nation. If it is not protected and improved, the chance that we shall be unprepared for a crisis involving survival, and undefended when the crisis arrives, becomes a frightening possibility.

In view of this, which is Finer's opening theme, the pertinent question is asked: What has happened to transform the office, so ingeniously devised by the Fathers and so successful in former years, into the institution of all others most in need of rescue from its present debilities? And the answer is, of course, that it is nothing that has happened to the office; it is what has happened all about the office to make its competence insufficient for its present and future responsibilities. In irrefutable arguments based on figures and historic trends, Finer shows why this is so. The amazing growth and increasing complexity of the nation's internal problems, as well as its position among the other nations, has loaded the Presidency with burdens no man could possibly carry alone.

No man could carry them. But there have been developments which have made it less likely that men who most closely approach the stature needed will be chosen for the office. The evidence for this is all too plain. If the first line of Presidents—Washington, Adams, Jefferson, Monroe, Madison, J. Q. Adams, and Jackson—is compared with the last group—(reading backward), Eisenhower, Truman, Roosevelt, Hoover, Coolidge, Harding, and Wilson—it is seen that giants in the White House are no longer so certainly insured by our system of selection.

The turn came when Jackson left office and the operations of the party system were institutionalized. Someone has said that the qualities it takes to be elected under that system inevitably disqualify the successful candidate for the Presidency. And there is something in the stricture. It is no longer, apparently, necessary to be born in humble circumstances and never to have become more distinguished in any quality than the most common voter. This was true from Harrison (the log-cabin and hard-cider candidate of the Whigs in 1840) to McKinley; but this commonness has now been replaced by an even more unreliable appeal—an appeal directed to the television audiences who can be fooled by the use of the actors' arts. An actor for President? That, say many of the critics, is what we are getting. There is no more objection, of course, to an individual because he is an actor than because he belongs to any other occupational group. But it ought not to provide him with a specious glamor. There ought to be some understood qualifications for the office which are related to its duties and responsibilities. These qualifications are hard to enumerate; but their lack is not at all hard to identify.

A sort of footnote to this is furnished by the weakness the electorate has always had for generals. There have been, among the 33 presidents, four recognizably professional ones—Washington, Taylor, Grant, and Eisenhower—and several others—such as Hayes, Garfield, and Pierce—who made much of their military records. If Washington is excepted on the ground that he was more statesman than soldier, and Jackson because he was an amateur, none has been a distinguished President and some have been our most deficient ones. This is said to underline the dangers of selection for *any* reason other than probable competence.

But the possibility of bad choices—perhaps probability would not be too strong a word—is given a disastrous emphasis by the performance, under stress, of the best of modern choices—say Wilson, Hoover, or Roosevelt. All were highly competent; all were industrious and dedicated; all were partial failures. Wilson had to give up his domestic program to give all his attention to the First World War, and he left much unfinished business that had to be the first responsibility of Roosevelt more than 20 years later. But Roosevelt had the same experience. After 1938, and until 1945, he simply had no time to give to anything except world affairs. There was a time when the Secretary of the Interior did not have an interview with Roosevelt for more than a year. And other Secretaries, if their difficulties were not quite so great, had much the same ones. As for Hoover, the problems of office left him quiveringly helpless while the country degenerated into chaos.

We need then, as Finer says emphatically, better men in office; but we also need a different kind of office, one that will give leadership, carry the burden of executing the laws faithfully, and make us secure in a parlous world. It can be achieved, he thinks, only by adopting the principle of collectivity. One man cannot do what has to be done. He suggests that 12 men, properly chosen and properly disposed, could. He would, therefore, abandon the kind of reforms that have been fashionable since Roosevelt's time—reforms that attempt to make the office efficient by furnishing the President with more and more assistance; all that does, Finer suggests, is to smother the President; and Finer further suggests that we try an entirely different approach.

All those people in the White House, now chosen by the single President, ought to have some kind of direct responsibility to the people. After all, we are a democracy. At least the chief assistants ought to be so chosen. Finer would have them be heads of departments, except for one or two who would be deputy or assistant Presidents. But it would be corporate cabinet. Its decisions would be collective ones, and all members would be responsible for them. As for their qualifications, Finer would make it necessary for all of them to be members of Congress or at least to have served four years in that body. Incidentally, this would, he feels, have a beneficial effect on Congress, about which he is even more caustic, in its present state, than he is about the Presidency.

There is more to this. Any critic who passes what might be called the constitutional barrier—that is, who allows himself to consider what he would recommend if the Constitution did not exist—is pretty certain to make some suggestions shocking to those who will not give themselves this freedom. These suggestions, moreover, involve not only the organ of government they have set out to consider, but all the others as well. The relations are too intimate for any to be considered in isolation. So Finer has a few suggestions to make about Congress as well as the Presidency. The Supreme Court—perhaps on the grounds that he had stirred up enough controversy already—he leaves pretty much alone. In the end, the debate will involve that body too.

What shall we say of these criticisms and suggestions? I prefer, at this moment, to say that the criticisms are valid. They do point to the need for change, and drastic change. As to the suggestions, it seems to me inevitable that we shall go back to the state of mind Madison was in a few months before the Constitutional Convention of 1787. Concerning the Executive, Madison said that he was not certain in his own mind whether he should be one man or whether he should be so situated as to be *primus inter pares*—the first among equals. How is that to be done? If we are to reverse the Fathers' judgment that the President should be one and try to make him a collectivity, how shall we dispose the relationships among the *pares* so that the Presidency will exhibit initiative, wisdom, and dedication; so that jealousies and ambitions will be minimized; and so that the public interest will become the first and last thought of everyone involved? That is the problem posed for us.

Herman Finer has made a suggestion. We shall hear from others. The first problem for the listeners, the undecided, in this debate will be to listen without prejudice, without preconception, and especially without constitutional blinders. It ought to be recalled that Washington, Franklin, Morris, Wilson, and all the others attendant on that Philadelphia conclave recognized no binding duty to the document under which they were *then* governed, and that if they had, they would have been stopped from producing the one we have been governed under ever since. Not even such charters are immune to the erosions of time and change.

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Africa Today and Tomorrow. An outline of basic facts and major problems. John Hatch. Praeger, New York, 1960. 289 pp. $4.

Events in Africa are moving much too rapidly for publication schedules to keep up with them. Inevitably, any book dealing with Africa which attempts to give an outline of basic facts and major problems is out of date before it can appear. *Africa Today and Tomorrow* was completed in the middle of 1959. Since then there have been major changes in almost every African country. The former French territories, with one exception, are now independent nations. Rioting in Nyasaland and more recently in Southern Rhodesia has raised questions about the long-term stability of the Federation of the Rhodesias and Nyasaland. A threatened African rebellion in the Union of South Africa has been crushed, but crushed in such a way that hostility between white and black has been exacerbated. The last few weeks have brought independence to the Belgian Congo, followed by violence, economic chaos, and intervention by the United Nations.

At the same time that the Africa which John Hatch wrote about has been changing rapidly, world interest in the affairs of that continent has become more urgent. More and more Americans are coming to feel that they need some guide to the continent which will help them to understand the reasons for African unrest, their demands for independence, and their needs for economic and technical assistance. Hatch has provided a succinct and informed account of recent African history which should be very useful as a background to the news reports that are now in the foreground. The focus is upon political and economic developments, each country being described in its turn. Hatch, who is an Englishman, has concentrated upon the regions where British interests have predominated. Some 175 pages deal with these areas. The discussion of the Portuguese territories and of the former territories of Belgium, France, and Italy is much sketchier. A final chapter, entitled "Future perspectives," attempts to predict the trends of the future. No one could quarrel with his conclusion that "All the signs in emergent Africa point to a revolutionary situation. . . . No one can doubt that during the next few years a series of revolutions will occur in every area of the continent—some constitutional, some violent, some in co-operation with the immigrant Europeans and Asians, others characterized by bitter racial antagonism."

Two useful appendixes conclude the book. One gives a brief chronology of major dates in African history; the other is a short gazette listing the various countries with their populations, principal products, form of government, and type of franchise.

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Prehistoric Investigations in Iraqi Kurdistan. Robert J. Braidwood and Bruce Howe. University of Chicago Press, Chicago, Ill., 1960. 184 pp. $5.

This book purports to be the story of a long-term field project aimed at shedding light on how settled village-farming communities first came into being; but all kinds of evidence from many major prehistoric and some early historic sites in southwestern Asia are presented and discussed in considerable detail by Braidwood and Howe. Specialized dating techniques, climatological evidence, paleoethnobotany, and the archeology of animal domestication are dealt with separately by Frederick R. Matson, Herbert E. Wright, Jr., Hans Helbaeck, and Charles A. Reed, respectively. Reed happily emphasizes the grave dangers of attempting to

extrapolate climatic sequences from phenomena observed in far-off Western Europe.

The senior authors draw a clear and coherent picture of cultural development in the area, from the assemblage of "pebble-tools, flake-tools, and Upper Acheulean type hand axes" which were still in vogue there "at the beginning of the Würm glaciation" or shortly before, through the following "Mousterian," "blade-tool," and microlithic industries down to the dawn of "incipient cultivation," which was heralded by the development of equipment for grinding grain and polishing stone celts and ornaments. They draw an interesting distinction between the "food gathering" procedures of "middle paleolithic" and the "food collecting" systems of "upper paleolithic" communities. They also note the presence of an apparent hiatus between the "era of incipient cultivation" and the emergence of "primary village-farming" communities; but they go on to show that this is only a gap in a demonstrable continuum, which can doubtless be filled by further excavations. What seems to me their most important conclusion is that "the transition to the food-producing" stage was not correlated with any "radical change in climate or fauna."

This is not an easy book to read, but it is invaluable for reference: no student of southwest Asian archeology can afford to pass it by. The maps are numerous and excellent, the illustrations good, and the presentation as a whole is highly satisfactory from the practical point of view.

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On Motion and On Mechanics. Comprising *De Motu* (c. 1590) (translated with introduction and notes by I. E. Drabkin) and *De Meccaniche* (c. 1600) (translated with introduction and notes by Stillman Drake). Galileo Galilei. University of Wisconsin Press, Madison, 1960. 193 pp. $5.

The publication of these two early treatises by Galileo, well translated and helpfully annotated, and with fine introductions, is indeed welcome. Those whose knowledge of Galileo has been confined to the two major Dialogues and the papers which Stillman Drake has previously translated (in *Discoveries and Opinions of Galileo*) will find in these texts the methods and concepts later sharpened and clarified by Galileo in his more mature works. There is a sense in which Galileo's biography synopsizes the transition from the old science to the new. The *De Motu* comprises not only a series of arguments directed against various aspects of Aristotelian mechanics, but is itself Aristotelian in style. Although a scholastic impetus theory prevails and Galileo still speaks of natural and violent motions, the method of analysis of real motions, which he later employs more successfully, is there.

Galileo's tortuous arguments in these treatises are necessary for airing a number of questions ancillary to the development of "two new sciences," for the death pangs of old theories are also the birth pangs of the new. For example, to what and how will mathematics apply, and how can experiment be used to decide upon certain problems? Here we see Galileo groping toward the necessary deployment of mathematical idealizations and toward the application of what Mach calls a "principle of continuity," which are perhaps the most important prerequisites to a mathematical science of nature.

Among the more interesting chapters of the *De Motu* are those in which Galileo employs the logical analysis of time and continuity in refuting certain Aristotelian views on motion. These enable us to understand why, although Galileo held so many mistaken and confused views at this date on such matters as the behavior of bodies in free fall and on inclined planes, he was yet ultimately able to achieve so much. For the essential conceptual clarifications were to follow upon repeated applications of these early critical methods.

Of interest in the *De Meccaniche* is Galileo's use of incomplete inertial and conservation principles, prior to their articulation as principles, restricted though their application may be. On the whole, these texts reveal the thorny road which is scientific inquiry, of which we cannot be too often reminded. I recommend them to the general reader as well as to the historian and philosopher of science. We are indeed indebted to I. E. Drabkin and Stillman Drake for executing admirably a difficult and worthwhile task.

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Marine Biology. B. [sic] N. Nikitin, Ed. *Transactions* of the Institute of Oceanology, vol. 20, U.S.S.R. Academy of Sciences Press, Moscow, 1957. American Institute of Biological Sciences, Washington, D.C., 1960. 302 pp. Illus. Nonprofit libraries and AIBS members, $7.50, others, $10.

This is one of a series of works being translated and published by the American Institute of Biological Sciences, apparently on a trial basis. Since the *Transactions* of the Institute of Oceanology comprise the most comprehensive journal of oceanography published in the Soviet Union, it is fitting that one of the volumes should be selected for translation. Marine Biology is reproduced from clear, original, typewritten sheets, but lacks running heads. The translation is accurate, the illustrations are well reproduced, and the subject matter of this particular volume is an excellent sample of the sort of marine biological work being done in the Soviet Union. There are papers on bottom communities by such authors as Savilov, Turpaeva, and Sokolov; plankton is the subject of papers by Ponomareva and Beklemishev; there are systematic reports on mollusks by Filatova, on hyperiid amphipods by Vinogradov, and a number of papers by various authors on the age and growth of fishes.

The individual or librarian who picks up this volume 10 years from now will be somewhat puzzled. There is no indication of editorial responsibility (for the translation), and the title page implies that the volume is published directly in English in cooperation (perhaps) with the Academy of Sciences of the U.S.S.R. Nowhere is there any overt indication that this is a translation, and there is nothing to indicate who is responsible for the translation. There is no statement that this is an isolated volume, not part of an entire English series of this journal. Furthermore, it is not only translated, but it is rigorously transliterated; not a single letter of the Cyrillic alphabet has been left anywhere. As a result, there is nowhere any indication of the proper Russian spelling of the authors' names; most notably, on the title page the name of the editor should be V. N., not B. N., Nikitin. This was carried all the way through the references (which for some reason are numbered seriatim in the translation, although this was not done in the original) with the result

that all names, titles, and journals have been reduced to the straightjacket of an American IBM typewriter. Anyone who does not have access to the original journal may have trouble identifying some of the authors. (Two names were differently spelled in the English contents of the original.) Since this volume is reproduced by photo-offset, it should have been no trouble to reproduce the Cyrillic bibliographies by facsimile. In contrast, minor errors have been retained in non-Russian citations. Most noticeable is the name of C. M. Yonge, which is Jonge in one paper and Ionge in another.

While these are minor defects that do not seriously interfere with the usefulness of this translation, it would not have cost any more, on such a large page, to include the Russian table of contents along with the English and to append a list of all proper names used in the text at the end of the volume. These are simply matters of cultural courtesy. Furthermore, the purchaser of such a volume as this is entitled to a little more information about the translation: Were all the papers translated by the same person, were they referred to the authors for criticism, and so on? Perhaps the AIBS should issue a supplementary page giving this information; such a page could be pasted into each volume in its series of translations. As it stands, this book, as useful and as welcome as it is, has a certain orphaned air about it.

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Economic Atlas of the Soviet Union. George Kish. University of Michigan Press, Ann Arbor, 1960. Illus. $10.

This is essentially a set of annotated maps, 60 of which represent 15 basic economic regions, with four maps for each region; the maps cover agriculture, minerals, industry, transportation, and cities. The last of these maps also marks the boundaries of the economic regions (*sovnarkhozy*) as they were in 1959.

Much ingenuity has been expended in devising ways of showing industrial location by conventional signs. The atlas has a number of positive features, but is open to criticism on several counts. For example, it is regrettable that there are no maps of soil and climate. If these had been included, the author might not have written that the rainfall of the Ukraine is "adequate to ample"; much of this area is subject to frequent droughts. It is also unfortunate that the colored maps of the U.S.S.R. on the inside of the cover mark a number of railway lines which are nonexistent and which are (rightly) not shown on the relevant regional maps (for example, a line north of Lake Baykal, parallel to the Trans-Siberian). It would also have been desirable to indicate the *sovnarkhoz* names. To take an example among many, Map 1D shows a region in which only one town, Saransk, is marked. I know enough of Russian administrative geography to be sure that there is no Saransk *sovnarkhoz*, but not enough to identify which *sovnarkhoz* it was without consulting another atlas. In the notes on Belorussia, it is wrongly stated that Belorussia was originally named "White" to distinguish it from "Muscovite or Red Russia"; Muscovy was never known as "Red." It is also a pity that, when there are many references to railways and pipelines under construction, nothing is said about the much-publicized plan to develop a vast new metallurgical center at Tayshet, in eastern Siberia.

A more technical problem is one of indicating the relative importance of the industries in various towns. Unfortunately, all the conventional signs are the same size, and large factories or centers are distinguished only by a somewhat darker print. As a result of this, the great textile center of Ivanovo looks no more significant than four places in the Ukraine, although in fact the Ukraine's textile output is quite small. That such things are not inevitable was shown in the Oxford regional atlas, *USSR and Eastern Europe*, published in 1956; the Oxford atlas was also superior in printing (many colors instead of black and brown only) and in arrangement.

George Kish has done a useful job, probably within a strictly limited budget, and the result, while certainly not perfect, will be helpful to students.

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New Books

Biological and Medical Sciences

Mitchell, J. S. *Studies in Radiotherapeutics.* Harvard Univ. Press, Cambridge, Mass., 1960. 281 pp. $9. The author is Regius professor of physics at the University of Cambridge. This book is based on the Dunham lectures which he delivered at Harvard University in 1958. It includes an account of the work in which Mitchell and his colleagues have been engaged since 1938.

Oberteuffer, Delbert. *School Health Education.* Harper, New York, ed. 3, 1960. 559 pp. $6.

Plunkett, Richard J., and John E. Gordon. *Epidemiology and Mental Illness.* Basic Books, New York, 1960. 143 pp. $2.75. This book is No. 6 in the series of monographs published by the Joint Commission on Mental Illness and Health.

Reitz, L. P., Ed. *Biological and Chemical Control of Plant and Animal Pests.* AAAS Symposium No. 61. AAAS, Washington, D.C., 1960. 286 pp. $5.75; prepaid order to members, $5. A symposium presented at the AAAS meeting in Indianapolis; the volume contains 19 papers on the recent advances in chemical control, on biological control, and on the public's stake in pest control.

Rose, Harry M., Ed. *Viral Infections of Infancy and Childhood.* Harper, New York, 1960. 255 pp. $8. Symposium No. 10 of the section on microbiology of the New York Academy of Medicine.

Sinnott, Edmund W. *Plant Morphogenesis.* McGraw-Hill, New York, 1960. 560 pp. $12.50.

Mathematics, Physical Sciences, and Engineering

Liebhafsky, H. A., H. G. Pfeiffer, E. H. Winslow, and P. D. Zemany. *X-ray Absorption and Emission in Analytical Chemistry.* Wiley, New York, 1960. 357 pp. $13.50.

Margen, P. H. *Nuclear Reactor Optimization.* Simmons-Boardman, New York, 1960. 91 pp. $2.75.

Nevins, Allan, *et al. Energy and Man.* A symposium. Appleton-Century-Crofts, New York, 1960. 126 pp. $3.75. Five papers on the production, use, and importance of energy in civilization; originally presented as addresses at a symposium sponsored by the Graduate School of Business, Columbia University, and the American Petroleum Institute.

Ordway, Frederick I., III, Ed. *Advances in Space Science.* vol. 2. Academic Press, New York, 1960. 463 pp. $13. The contents cover space physics, tracking, materials, electrical propulsion systems, and attitude control. The contributors include D. F. Lawden, J. R. Pierce, C. C. Cutler, J. H. Huth, E. B. Konecci, H. J. Schaefer, R. G. Toscher, and H. H. Koelle.

Ratcliffe, J. A., Ed. *Physics of the Upper Atmosphere.* Academic Press, New York, 1960. 598 pp. $14.50.

Shubnikov, A. V. *Principles of Optical Crystallography.* Translated from the Russian. Consultants Bureau, New York, 1960. $9.50.

Stephenson, Reginald J. *Mechanics.* And properties of matter. Wiley, New York, ed. 2, 1960. 377 pp. $7.50.

Termier, Henri, and Geneviève Termier. *Erosion et sédimentation.* Introduction à la géologie générale et à la paléogéographie. Masson, Paris, 1960. 412 pp. NF. 69.

Reports

Interocular Transfer of Brightness Discrimination in "Split-Brain" Cats

Abstract. "Split-brain" cats with midsagittal section of the optic chiasm and the corpus callosum are able to transfer interocularly a simple brightness discrimination although they are unable to transfer a pattern discrimination. This strongly suggests that the interocular transfer of a simple brightness discrimination is mediated subcallosally.

Recent experimental work by Sperry *et al.*, by Myers, and by Downer (*1*) has demonstrated that cats and monkeys that have had the optic chiasm and the corpus callosum sagittally sectioned in the midline fail to transfer to the untrained eye visual pattern discriminations learned with the other eye. These experiments implicate the corpus callosum as the interhemispheric commissure important for the interocular transfer of pattern discriminations. No systematic observations, however, have been reported on the interocular transfer of brightness discrimination or other types of visual discriminations in the "split-brain" cat, although Schrier and Sperry (*2*) have suggested that the transfer of brightness discrimination might be different from pattern discrimination. The present experiment was designed to determine if a simple, suprathreshold visual brightness discrimination would transfer interocularly in "split-brain" cats that failed in the interocular transfer of a visual pattern discrimination.

Six experimentally naive cats were used. Prior to all training, each animal had the optic chiasm sectioned sagittally by the transbuccal approach, followed in 14 to 35 days by section of the corpus callosum. The right cerebral hemisphere was retracted during callosal surgery in all animals. All abnormal postoperative neurological signs were transitory and abated within one week. A 2 to 4 week postoperative period intervened before training and testing were begun.

All training and testing were performed in a semidarkened, food-approach discrimination apparatus. The animals were trained to select the correct one of two adjacent, translucent, plastic panels illuminated from the rear and hung at the end of an alley 2½ feet long. Correct responses were rewarded with a small piece of meat, and the animal was permitted to correct errors. All training and testing, after initial shaping in the discrimination apparatus, were performed with one of the cat's eyes covered by a mask of the type devised by Myers (*3*).

During pattern-discrimination training and testing, both plastic doors to which the stimuli were affixed were illuminated to the same intensity. To minimize differences in brightness cues between the patterns used, the positive and the negative stimuli were respectively black upright and inverted equilateral triangles of the same area. During brightness discrimination training and testing, one plastic door remained dark, while the other door was illuminated from the rear by a 40-watt incandescent bulb producing a brightness of 3.27 ca/ft.^{2} at the surface of the door. The brightness at the surface of the nonilluminated door was 0.24 ca/ft.^{2}. In both brightness and pattern discrimination trials, the positive and negative stimuli were alternated from right to left according to the Gellerman sequence (*4*).

Four cats were trained and tested first in the brightness discrimination and then in the pattern discrimination, and two cats were trained and tested in the reverse order. The training schedule consisted of 5 to 7 daily sessions each week with 30 to 50 trials daily. With three cats the upright triangle was the positive stimulus, and with the other three cats the inverted triangle was positive. Because cats generally prefer the illuminated door in this brightness discrimination procedure, all animals were tested for interocular transfer with the darker door as the positive stimulus. Three of the animals were initially trained with the illuminated door as the positive stimulus and were then subjected to stimulus-reversal training before interocular transfer was tested.

The criterion for successful initial learning and for successful interocular transfer was designated as 18 or more correct choices among the final 20 trials on one day and 18 or more correct choices among the initial 20 trials on the next succeeding day, the total over-all percentage of correct responses on both days being 90 percent or better. No overtraining beyond criterion was carried out in either testing procedure.

Of the six cats prepared for this experiment, two transferred the pattern as well as the brightness discrimination at or near criterion levels, and gross inspection of their brains revealed incomplete chiasmal sections although the corpus callosum was completely sectioned in both of these brains. The data on these two animals are not reported here.

The four remaining animals satisfied our behavioral criterion by failing in the interocular transfer of a pattern discrimination. Each of these four cats, however, transferred the bright-

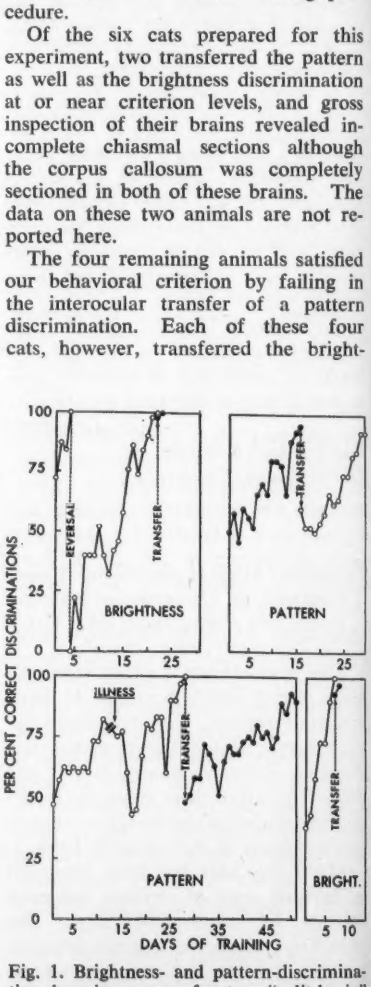

Fig. 1. Brightness- and pattern-discrimination learning curves for two "split-brain" cats. Upper two graphs, animal No. 1; lower two graphs, animal No. 2. Open circles indicate left eye training; closed circles indicate right eye training.

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [*Science* **125**, 16 (1957)].

ness discrimination at or above criterion levels, regardless of whether the brightness discrimination testing was conducted before or after the pattern discrimination testing and regardless of the order in which the eyes were trained and tested in the two discriminations.

These results are shown in Fig. 1. Inspection of the learning curves for pattern discrimination with each eye shows that the "split-brain" cat essentially relearns with the untrained eye. Furthermore, the course and rate of the learning with the untrained eye closely approximates that of the initially trained eye. Pattern-discrimination learning is impaired in both eyes, however, for when compared to the monocular learning of the unoperated cat, the "split-brain" cat requires 2 to 5 times more trials to reach criterion. Brightness discrimination, on the other hand, not only transfers completely from one eye to the other in the "split-brain" cat, but learning of the discrimination proceeds about as rapidly in this cat as in the normal cat restricted to monocular vision.

Because the four animals reported in this experiment are still being tested in other visual problems, the extent of their lesions has not yet been determined. Nevertheless, these results demonstrate that in cats with mid-sagittal section (of undetermined completeness) of the optic chiasm and the corpus callosum, a simple, suprathreshold brightness discrimination will transfer interocularly whereas a pattern discrimination will not. This suggests either that in cats the corpus callosum is not essential in mediating the transfer of a simple brightness discrimination or that less functional corpus callosum is necessary for brightness transfer than for pattern transfer.

Smith (*5*) has reported that simple brightness discriminations can be performed in the cat after ablations of the striate cortex; this indicates the capabilities of undetermined other cortical or subcortical structures in visual discriminations. Bridgman and Smith (*6*), however, have shown that with brightness discriminations at or near threshold levels in the cat, the striate cortex is involved and thus is essential for critical performance at these levels. Hence, if the chiasmal and callosal sections are complete in the animals reported here, the present results indicate that a simple, suprathreshold brightness discrimination is transferred by undetermined subcallosal commissures. Therefore, the corpus callosum may be essential only for the transfer of brightness discriminations at or near threshold, a cortically dependent visual function.

These interpretations suggest two sequel experiments which are now in progress utilizing "split-brain" cats: (i) testing interocular brightness discrimination transfer near threshold levels, and (ii) testing interocular brightness discrimination transfer after additional subcallosal midline-sections of other commissures (*7*).

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7. This work was supported by U.S. Public Health Service grant 2B-5273. The authors are indebted to Dr. Eliot Stellar for his advice throughout this experiment.

17 June 1960

On the Reported Inhibition of Monoamine Oxidase by an Agent with Sedative Properties

Abstract. 1-Benzyl-2-methyl-5-methoxytryptamine has been reported, on the basis of indirect evidence, to inhibit monoamine oxidase. More direct experiments, however, demonstrate that the drug is devoid of the ability to block monoamine oxidase in brain in vitro or in vivo.

Considerable evidence supports the hypothesis that the antidepressant action of monoamine oxidase inhibitors is associated with their ability to inhibit the enzyme in brain (*1*). Based on this hypothesis there have been developed a number of potent monoamine oxidase inhibitors (for example, iproniazid, phenylisopropylhydrazine, phenelzine, nialamide) for the treatment of depressed mental conditions. Feldstein *et al.*, in a recent report (*2*), question whether the antidepressant effect produced by such compounds is related to inhibition of monoamine oxidase. Their objection to this view is based on evidence that 1-benzyl-2-methyl-5-methoxytryptamine (BAS), though a sedative agent, blocks monoamine oxidase. As evidence that this sedative agent inhibits monoamine oxidase they reported that the pretreatment of schizophrenic patients for 1 to 2 weeks with about 1.5 mg of the drug per kilogram per day prevented the expected rise in urinary 5-hydroxyindoleacetic acid after the administration of DL-5-hydroxytryptophan. They concluded that their data cast doubt upon the hypothesis that there is an association between the central stimulatory effects of monoamine oxidase inhibitors and their ability to inhibit monoamine oxidase. However, they also reported that BAS did not reduce the excretion of endogenously formed 5-hydroxyindoleacetic acid. In view of the nature of their indirect and contradictory evidence we undertook to determine by direct means whether the sedative blocks the activity of brain monoamine oxidase.

Previous studies have shown that iproniazid and other potent monoamine oxidase inhibitors interfere with the destruction of serotonin and norepinephrine in rabbit brain, thereby causing a two- to threefold elevation in the levels of these amines (*1*). 1-Benzyl-2-methyl-5-methoxytryptamine was given intravenously to rabbits in daily doses of 1 to 2 mg/kg for 10 days. The levels of the brain amines, as measured by previously described techniques (*3*), were not increased over the normal values during the 10 days of drug administration. Increasing the daily dose of the sedative to 25 mg/kg also failed to elevate the levels of these brain amines.

The inhibitory action of BAS on monoamine oxidase in vitro was assayed by its effect on the metabolism of serotonin added to rabbit brain homogenates (*4*). At concentrations as high as $10^{-4}M$ BAS failed to inhibit monoamine oxidase; contrastingly, iproniazid at $10^{-4}M$ or β-phenylisopropylhydrazine at $10^{-5}M$ completely suppressed the activity.

These results indicate that 1-benzyl-2-methyl-5-methoxytryptamine has little or no activity as an inhibitor of brain monoamine oxidase in vitro or in vivo. It is not surprising, therefore, that the compound does not show antidepressant properties (*5*).

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2 June 1960

Spiral Cloud Band Associated with a Tornado

Abstract. The radarscope photographic sequence, by far the best example on record, reveals a spirally banded structure about 10 miles in diameter which is similar to spiral bands of hurricanes ranging as far as 250 miles and to the banded cloud structure of well-developed storms of 1000 miles as photographed by Tiros I.

On 19 May 1960, the radar at the U.S. Weather Bureau Office, Topeka, Kansas, tracked a devastating tornado that cut a path over 100 miles long across northeastern Kansas, where the town of Meriden suffered the greatest destruction. Two scientifically noteworthy features of the radar observations were: (i) indications on the range-height-indicator scope that the top of the tornado's parent cloud was above an altitude of 70,000 feet and (ii) pictures of the plan-position-indicator scope that show the best example yet recorded of a cyclonically spiraling hook-shaped echo associated with a tornado.

The first observation of a radar hook echo associated with a tornado was reported in 1953 by Stout and Huff (*1*) and was later carefully analyzed by Fujita (*2*). Fujita found that the spiraling of the hook echo cyclonically inward was produced by a small cyclone approximately 30 miles in diameter and "resembling a miniature hurricane in many respects." Figure 1 shows Fujita's outline of the radar echo of the parent cloud and its hook-shaped appendage. A sequence of pictures of this original observation showed that the hook formed in about 13 minutes on the right rear quadrant (relative to direction of movement) of the large echo of the parent cloud and curled cyclonically inward.

Although many radar observations of this phenomenon have been recorded (see *3*) since the original observation by Stout and Huff, the observation of the spiraling hook associated with the Meriden tornado is noteworthy because it is by far the best example on record. Time-lapse photographs of the plan-position-indicator scope show conclusively that the hook formed in

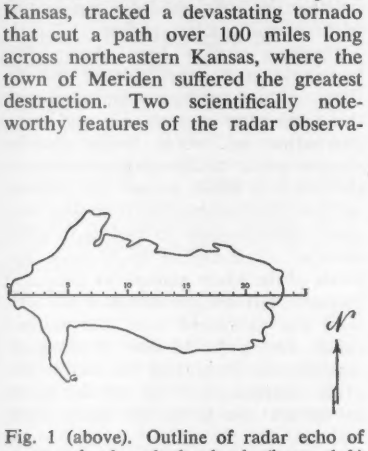

Fig. 1 (above). Outline of radar echo of parent cloud and the hook (lower left) associated with a tornado at Champaign, Ill., 9 Apr. 1953. The tornado was located near the tip of the hook, which was curling cyclonically as indicated by the arrow. [After Fujita (*2*)]

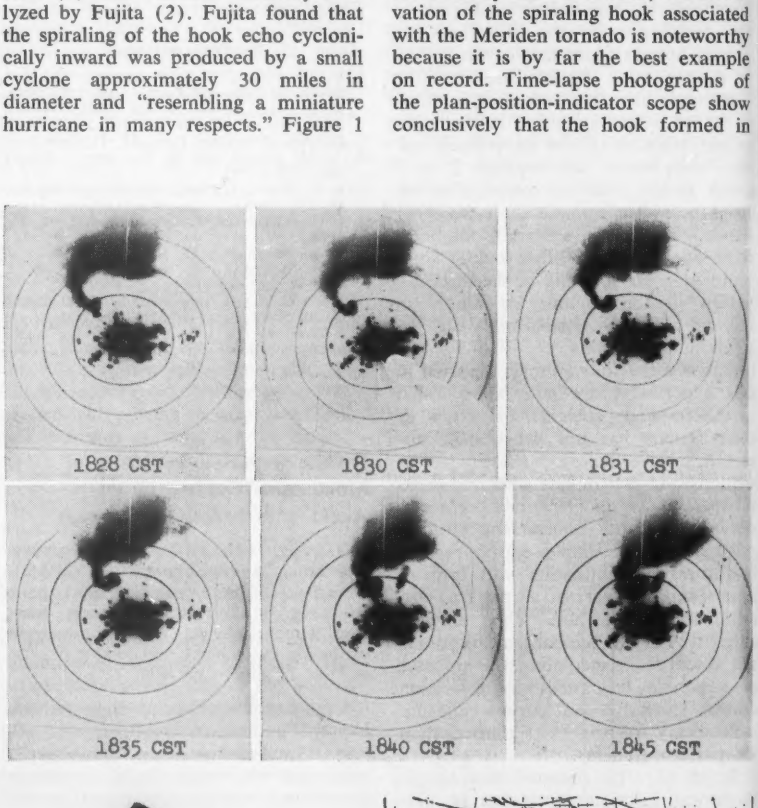

Fig. 2 (right). Sequence of photographs of the Topeka, Kan., plan-position-indicator radar scope, showing the cyclonic inward-spiraling hook echo associated with the Meriden tornado, 19 May 1960. The hook protrudes from the right rear (lower left) quadrant of the parent cloud. The scale is indicated by the circular markers at 10-nautical-mile intervals.

Fig. 3. Picture of radar scope at Weather Bureau Office, Cape Hatteras, N.C., showing hurricane Helene at 1705 EST, 27 Sept. 1958. Note the cloud bands spiraling inward to a closed "wall cloud" surrounding the hurricane "eye." The circular markers are at 50-nautical-mile intervals with an intermediate marker through the eye at the 72-nautical-mile radius. [After Sadowski (*7*)]

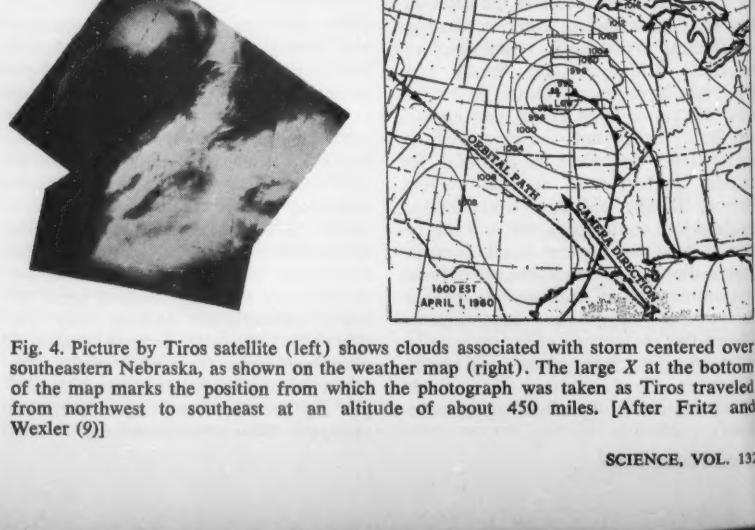

Fig. 4. Picture by Tiros satellite (left) shows clouds associated with storm centered over southeastern Nebraska, as shown on the weather map (right). The large *X* at the bottom of the map marks the position from which the photograph was taken as Tiros traveled from northwest to southeast at an altitude of about 450 miles. [After Fritz and Wexler (*9*)]

a cyclonic flow. The photographic sequence reproduced in Fig. 2 reveals the spiral motion of the well-defined hook echo as it curled cyclonically inward to a center in the right rear quadrant of the parent cloud.

The dimensions of the small cyclone (mesoscale low) that apparently curled the hook into a tight spiral have not been determined from analysis of the weather observations, but the curvature of the hook and of spiral streamers imbedded in the parent cloud suggests a diameter of the same order of magnitude (10 miles) as that of the "tornado cyclone" discovered by Brooks (*4*) in 1949.

These radarscope photographs come at an opportune time, when meteorologists are becoming keenly interested in the spirally banded structure of weather phenomena at all scales. The spiral bands of hurricanes observed on radar, as shown in Fig. 3, are well known (*5–7*). These spiral bands extend in length up to 250 miles (*6*).

Recently Tiros I (experimental weather satellite) revealed that spiral cloud bands also exist around well-developed storms located outside of the tropics (*8*, *9*). In these storms the bands, separated by clear areas, range in width from several miles to a few hundred miles, and in length they range to about 1000 miles. Such a spiral band is shown in Fig. 4 in a photograph taken by Tiros of a well-developed storm centered over Nebraska at 3:30 P.M. EST, 1 April 1960. The center of the storm was marked by a circular cloud mass in the upper part of the photograph, with an extension of the spiral along a cold front trailing from the storm center to the south and southwest, as shown on the surface weather map of Fig. 4.

Striking similarities can be seen in these observations (Figs. 1–4) of cyclonically spiral bands associated with atmospheric vortices ranging in size from the small scale of a 10-mile hook echo of a tornado, through the medium scale of the 250-mile-long spiral band of a hurricane, to the large scale of the 1000-mile-long frontal system of an extratropical storm. The atmospheric processes which produce these bands are believed to be different at each scale, but the similarity of appearances may reflect some similarity of kinematics. To the extent that this may prove to be true, there is hope that increased understanding of any one of the phenomena will contribute to our progress in understanding the others (*10*).

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5 July 1960

Electrical Responses of the Retinal Nerve and Optic Ganglion of the Squid

Abstract. Recordings were obtained from the retinal nerves and optic ganglia of intact squid, which were maintained in good condition by perfusing their mantles with sea water. Only "on" discharges were found in the nerves, whereas "on" and "off" discharges as well as spontaneous activity and tactile responses were obtained from the ganglia.

Several highly competent investigators (*1*), as well as one of us (E. F. M.), have repeatedly tried without success to record impulses from the retinal nerve fibers of cephalopod mollusks. There appeared to be two possible reasons for this lack of success: (i) impulses are not discharged in this nerve; (ii) the retinal nerve may be unusually sensitive to injury or ischemia produced after interruption of the circulation.

It has been suggested (*2*) that impulse propagation is unnecessary in short axons only a few space constants in length. It is entirely conceivable that the large electrical changes known since the time of Fröhlich (*3*) to take place in the receptors could be conducted decrementally in the optic nerve fibers, which are the axons of these cells, and retain sufficient amplitude to cause excitation in the optic ganglion.

Evidence of this mode of transmission in the squid's retinal nerve would furnish a concrete example of an important phenomenon which has long been postulated but has never been established by direct experiment.

It occurred to one of us (W. E. L.), in the course of bleeding squid to obtain hemocyanin for x-ray crystallographic studies, that it should be possible to maintain the squid in good condition by perfusing the gills with sea water while probing the retinal nerve with a microelectrode, and thus to determine whether conduction in this nerve takes place by the discharge of impulses or by electrotonic spread. A similar technique has been used for studying the giant axon in vivo (*4*).

In our experiments the squid (*Loligo pealii*) were placed on their backs, and their mantles were wrapped in a jacket of sheet lead. Two plastic tubes were inserted into the mantle through the valve on either side of the head. Sea water at 17°C flowed into these tubes and out the siphon, which could be deflected to one side by means of a lead hook to uncover the region over the optic nerve and ganglion. The tentacles were immobilized with a lead strap. An animal in good condition exhibited regular breathing movements and ejected a jet of water violently when stimulated.

Potentials were recorded between a glass-coated metal microelectrode (*5*), inserted into the head of the animal, and the lead jacket. A capacitance-coupled amplifier, dual-channel oscilloscope, and camera were used for recording, and an audio amplifier and loud-speaker were used for monitoring the experiment.

A stimulator comprising a tungsten lamp, electromagnetic shutter, neutral density wedge, and lens system was used to project a spot of light on the eye. A photoelectric cell was used in conjunction with one trace of the oscilloscope to indicate the duration of the stimulus.

Under suitable illumination the retinal nerves and optic ganglia could be seen through the transparent, cartilaginous "skull." A strong, steeply tapered electrode could be inserted manually through the skull, which then held it in place. Under these conditions impulse discharges from several units were recorded simultaneously. It was not possible to manipulate the electrode with sufficient precision to record from single units in isolation. It was also possible to punch holes in the skull with an 18-gauge hypodermic needle to permit the insertion of more fragile microelectrodes and still maintain the squid in good condition for an indefinite period.

To make certain of the exact location of the tip of the microelectrode in some experiments, the cartilage and muscle overlying the retinal nerve and ganglion were dissected completely away before insertion of the electrode. Results were identical to those obtained with an intact skull, except that survival time was only a few minutes.

As shown in the top trace of Fig. 1,

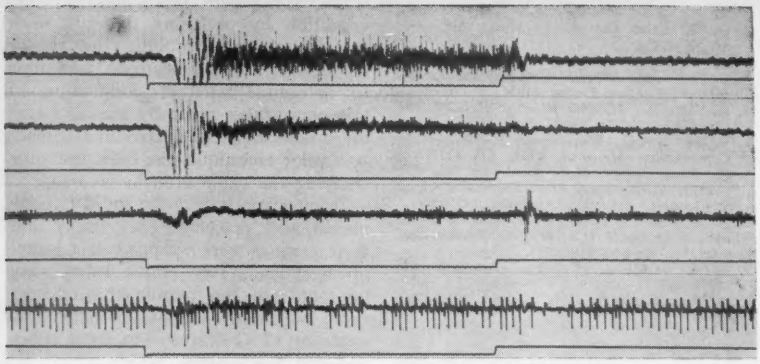

Fig. 1. Electrical activity of the retinal nerve and optic ganglion of the living squid. A glass-covered platinum 30-percent iridium microelectrode was applied directly to the surgically exposed nerve and ganglion. (Top trace) Electrode on nerve at point of emergence from retina; (2nd trace) electrode on surface of ganglion; (3rd trace) electrode penetrating ganglion; (bottom trace) electrode deep in ganglion. The bottom line of each trace shows the signal from the photoelectric cell; downward deflection indicates light on. Duration of stimulus, 0.51 second; amplifier coupling time constant, 0.001 second; amplitude of spikes in top record, approximately 500 μv.

the retinal nerve gave a discharge of impulses with a latency of about 0.05 second after the stimulating light was turned on. The frequency of impulses decreased within less than a second to a steady, maintained value and ceased abruptly after the stimulus was turned off. A damped train of slow oscillations appeared at the beginning of the discharge. These oscillations are of unknown origin and appear to be similar to those described by Fröhlich in the electroretinogram of *Eledone* (*3*), and by Wagner and Wolbarsht (*1*) in the octopus. They had many times the amplitude of the impulses from the nerve fibers, even though a millisecond coupling time constant was used in the amplifier to remove the slower components of the electroretinogram. No "off" discharges were found in the retinal nerve.

The discharge recorded from the surface of the optic ganglion (Fig. 1, second record) was identical to that recorded from the nerve and was presumably due to the fibers which cross the surface of the ganglion before entering it. Within the ganglion, both "on" and "off" discharges (third record) were obtained, similar to those found by Wilska and Hartline (*5*) in the optic lobe of *Limulus*. There was also a great deal of impulse activity within the ganglion, which was of spontaneous or undetermined origin (fourth record), though tactile responses were sometimes obtained. Obviously the two optic ganglia, which comprise the largest portion of the squid's central nervous system, are concerned with more than purely visual responses.

In these preliminary experiments (*6*) it was not possible to obtain quantitative information from single units, as has been done so successfully in *Limulus* and in the vertebrates. We only attempted to show that the retinal nerve of the squid discharges impulses in response to light and, in general, behaves as might be expected for the axon of a primary photoreceptor. We hope that others will be encouraged to make quantitative studies on the responses of the visual system and will solve the problem of keeping the microelectrode rigidly fixed with respect to the head, so that responses from single units can be recorded for an indefinite period. We also hope that our success in keeping the squid intact though immobilized for an indefinite period will encourage the study of the physiology of the circulation, the central nervous system, and the other sense organs of this intricate and highly organized animal.

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7 July 1960

Factors in Phoretic Association of a Mite and Fly

Abstract. Combined rearing of the mite *Myianoetus muscarum* (L.), and the fly *Muscina stabulans* (Fall.) has revealed adaptations of the hypopus to a series of fly factors. These adaptations favor the mite's dispersal. Hypopi are attracted to the pupa by a volatile substance and cluster on the anterior end, from which the fly emerges.

Anoetid mites share with some other mite families a distinctive stage in the life cycle, the deutonymph, which is highly adapted for dispersal by insects. The deutonymph, or hypopus, is an alternative stage interposed between protonymph and tritonymph. Conditions eliciting its appearance are as yet poorly understood, and may involve genetic factors besides physicochemical factors of its environment. Adaptations of the deutonymph include increased sclerotization, reduction in leg size, anal suckers, and rudimentary mouthparts (*1*). A marked degeneration of the digestive tract of *Histiostoma laboratorium* Hughes has also been observed (*2*, p. 122). The hypopus of *Myianoetus muscarum* (L.) was first noted on the housefly in 1735 (*3*). Berlese (*4*) reared the adult from hypopi found on *Muscina stabulans* (Fall.), and recently Cooreman (*5*) redescribed the species. We know of no work in which this or any other species in the family has been raised with fly larvae for the purpose of studying phoretic behavior. This report describes a series of orientations of the hypopus of *Myianoetus muscarum* which are behavioral counterparts of the anatomical adaptations noted above.

Trapped flies bearing hypopi were placed in a fly cage and maintained on lump sugar and water. Maggots and mites were reared at 24°C on meat covered with moist sawdust. Oviposition and pupation of the fly also occurred in this medium, as well as the complete mite life cycle. As a prelude to pupation, the prepupa usually faces downward, and in this position begins to construct a cocoon of cemented sawdust. Presumably, salivary secretions constitute the cement, since the salivary glands are greatly distended in the prepupa, whereas in actively feeding maggots and newly formed pupae they are much smaller (*6*). The prepupa then inverts, completes the top of its cocoon, and pupates soon after. Inversion of the jar elicits immediate reorientation of prepupae. Noteworthy is the normal sequence of positive and negative geotropisms which occurs within the space of a few hours.

Clusters of hypopi which may contain thousands of mites are often found

Table 1. Summary of data on the attraction of hypopi to a volatile substance produced by pupae of *Muscina stabulans*.

Groups*	Number of observations	Average number of hypopi (±S.E.*)	Range
Exposed to light			
A. Tubes with pupae	33	30.2±5.2	10–150
B. Tubes without pupae (control)	25	3.5±0.5	0–8
Exposed to dark			
C. Tubes with pupae	19	18.8±3.4	7–64
D. Tubes without pupae (control)	16	1.3±0.4	0–5

* Significance: between groups A and B, $p < 0.001$; between groups C and D, $p < 0.001$; between groups A and C, $p > 0.1$.

within the cocoons and on the pupal case. The cocoons are mite-tight but for a few interstices through which mites can enter. Hypopi are not attracted to the prepupa within its cocoon. Less than an hour after pupation, however, they begin to cluster on the anterior end of the pupa. In moderately heavy aggregations, mites pile up three or four deep on the anterior third of the pupa (Fig. 1), while in occasional very heavy infestations they may cover the entire pupa. Those in contact with the puparium are quiescent; the others are more active and mobile. No other stage of the mite is attracted to the pupa, nor is the hypopus attracted to the food which attracts the other stages. It was the regular occurrence of these aggregates on pupae which led us to test for the presence of an attractant.

Fig. 1. Pupa of *Muscina stabulans* with hypopi typically aggregated at cephalic end.

Pupae were washed free of mites in cold running tap water, dried on paper toweling, and placed around the periphery of a piece of dry filter paper cut to fit the bottom of a 15-cm petri dish. Hypopi were taken from the medium and deposited at the center of the paper. The dish was covered and kept at 22° C and 20 percent relative humidity. The following observations are based on this method. The attractivity of individual pupae varies, but usually, after 1½ hours, about 30 to 50 hypopi collect on the anterior end of the pupa; about one-fifth as many are found on the rest of the pupa. The following factors had no effect on the polarity of attraction: age of pupae, plane of orientation, and direction or absence of light.

Pupae of several fly species maintained in our laboratory were similarly treated and placed in alternating sequence around the petri dish. Pupae of *Stomoxys calcitrans* (*7*), *Musca domestica*, and *Lucilia sericata* were attractive, and those of *Fannia canicularis* and *Drosophila melanogaster* were not. These results are confirmed by our observations of mite and fly associations in the laboratory media. Pupae of *Muscina stabulans* which were killed with cyanide gas or washed in acetone and then thoroughly air dried retained their attractivity. Those which were boiled in water or acetone for 15 minutes, rinsed several times, and thoroughly air dried had no attraction.

The volatility of the attractant was established in the following way. Plastic tubes, with an inside diameter of 7 mm, were diagonally cut at each end so that the side view had the form of a trapezoid. This provided sloping ends and maximum and minimum tube lengths of 28 mm and 8 mm, respectively. Eight pupae of *M. stabulans* were washed free of mites, dried, and placed in a tube. The tube was closed at each end with a piece of filter paper affixed with paraffin. Controls contained no pupae but were otherwise the same. Three controls and three tubes with pupae were placed alternately in the petri dish setup described above. With the aid of a dissecting microscope, hourly counts were made of mites on each end of the tube. After each count, the mites were brushed from the ends of the tubes and the tubes placed in different positions in the dish. Series of counts were recorded for both light and dark situations. The experiment was repeated three times with different pupae and tubes. The data indicate that pupae produce a volatile substance which is attractive to hypopi (Table 1). Mites are attracted as readily in the dark as in the light (*8*).

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13 July 1960

Licking Rates of Albino Rats

Abstract. Local licking rates of eight albino rats, when the rats were given tap water at the end of a short runway after 23 hours of water deprivation, averaged a little over seven licks per second. Small variations within sessions were found, rates being slightly but consistently higher at the beginning than later on in sessions. Inter-session and individual differences were also observed, but were only of the order of one lick per second. Within practical limits, it appears that licking rates of rats are constant.

When licking rates of four albino rats were measured by means of a cumulative recorder, it was reported that "the local rate of drinking an acceptable solution was constant for each animal," and that the "local rates of responding remained between five and six licks per second for all animals under all conditions" (*1*). The animals drank by licking the fluid under test from a drinking tube with a 3-mm opening. The data reported were utilized to show that local licking rate, or rate of consummatory behavior, is not a function of size of reward and hence cannot be related to other behavior variations which do accompany changes in reinforcement magnitude (*2*).

The statements about the constancy of licking rates are, however, imprecise.

In the one case licking rates are said to be *constant*, but specification of this constant is left at "between five and six licks per second." We are, then, obliged to specify more precisely the "degree" of constancy of licking rates of rats over a period of time, and also to present data, lacking in the earlier report, on variations between animals.

Four male and four female albino rats from the colony maintained by the University of Sydney were used in the experiment. The animals were approximately 180 days old at the beginning of the experiment, 5 of them being studied over a period lasting 38 days. The apparatus was an enclosed straight alley 1 m long; a drinking tube with a 4-mm opening was inserted at one end. When an animal reached this end it stood on a metal plate from which a small electric current passed through the rat whenever its tongue touched the tap water in the tube. This current was amplified to activate a Both polygraph which recorded tongue laps on a paper moving at 5 cm/sec. Before each test the animals were deprived of water for approximately 23 hours. The experimental room was air-conditioned, and the temperature was maintained between 81° and 83°F.

As in earlier studies (*1*, *3*) the animals engaged in alternate bouts of licking and resting. Figure 1*a* shows the distribution of mean local rates of licking for all eight animals over a period of 5 successive days. Each animal was left in the apparatus for 5 minutes and permitted to drink or explore freely. The data shown in Fig. 1*a* were computed by dividing the total number of licks made by each animal in a session by the total time spent licking in that session. Intrasession variability data are presented in Fig. 1*b* in the case of two males. These graphs are based upon the licking rates of the animals in each run of licks lasting 1 second or longer. Thus rat M4 engaged in 24 bouts of licking lasting at least 1 second, and rat M1 licked for at least this time on 13 occasions, and the rates in each session ranged from 6.0 to 7.8 and 6.0 to 7.0 licks per second for rat M4 and rat M1, respectively. For both animals the fastest rates occurred near the beginnings of the sessions. The two fastest rates for rat M4, 7.8 and 7.6 licks per second, were the rates of its first two bouts of licking. The fastest rates for rat M1 were on its second and third licking runs.

The above data include all the tongue laps made by the animals in the particular sessions. They incorporate a slight degree of error because some records were not clear enough to allow accurate counts to be made throughout their length. To remedy this and to check on the possibility that systematic variations occurred from the beginning to the end of each session, the records were examined by counting the number of licks made in 5-second periods in clear parts of the beginning, middle, and end of each record.

Of 76 complete triplets so obtained from all eight animals over 5 successive days, and from five of the animals on nine other occasions spaced irregularly over a period of 23 days, the number of responses made in the 5-second period taken from the beginning of the record was larger than the number in the final period on 59 occasions, equal to it 9 times, and smaller only 8 times. These data are highly significant and show that licking rates are faster at the beginning than at the end of sessions. However, the absolute difference is small. Averaged across all animals from the total 76 triplets, the rate dropped from 7.67 licks per second at the beginning to 7.14 licks per second at the end of the sessions.

The distributions of licks per 5-second period for the extreme animals are shown in Fig. 1*c*. The mean rate over all sessions ranged from 6.64 licks per second for rat F4 up to 7.52 for rat F1. The difference between licking rates of these two animals is statistically significant ($p<.001$). The mean licking rate for all the animals in the first 5 days of the experiment was 7.03 licks per second. The mean licking rate derived from 43 samples of 5-second licking periods of five animals after about a month's training was 7.52 licks per second. Hence there is little variation in licking rate over time, and the licking rate of just over 7 licks per second is a little higher than that reported by Davis and Keehn (*1*). This difference might be due to the difference in drinking tube aperture, 3 mm versus 4 mm, in which case the volume of fluid obtained per lick might affect rates of licking, or it might be a function of strain differences, or it might result from the different recording techniques used in the two experiments.

In summary, the licking rate of rats when they drink is not *absolutely* constant, although variations are small. Rates of lapping vary within sessions and between sessions for individual animals, and between animals, in a

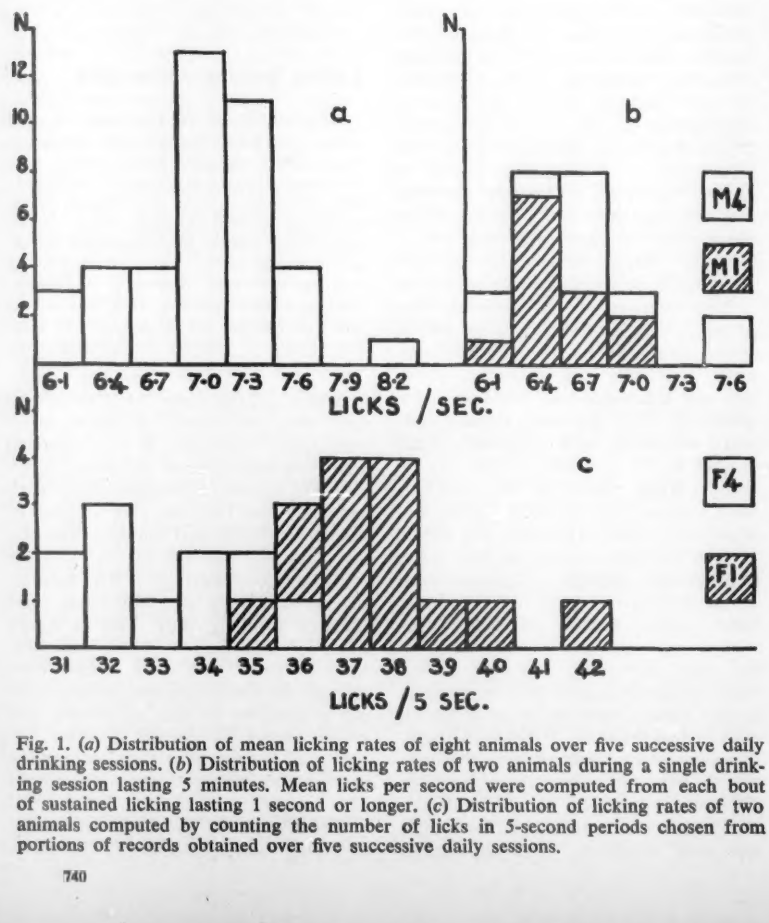

Fig. 1. (*a*) Distribution of mean licking rates of eight animals over five successive daily drinking sessions. (*b*) Distribution of licking rates of two animals during a single drinking session lasting 5 minutes. Mean licks per second were computed from each bout of sustained licking lasting 1 second or longer. (*c*) Distribution of licking rates of two animals computed by counting the number of licks in 5-second periods chosen from portions of records obtained over five successive daily sessions.

consistent way. However, these variations are extremely small compared with those found along other dimensions of behavior. The fastest and slowest drinkers in the present sample differed by less than 1 lick per second, on the average, and individual variability was of the same order of magnitude (*4*).

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23 May 1960

Some Moral and Technical Consequences of Automation—A Refutation

Abstract. The machine is not a threat to mankind, as some people think. The machine does not possess a will, and its so-called "conclusions" are only the logical consequences of its input, as revealed by the mechanistic functioning of an inanimate assemblage of mechanical and electrical parts.

In an article entitled "Some moral and technical consequences of automation" (*1*), Norbert Wiener has stated some conclusions with which I disagree. Wiener seems to believe that machines *can* possess originality and that they *are* a threat to mankind. In ascribing a contrary opinion to the man in the street—to wit, "that nothing can come out of the machine which has not been put into it"—he overlooks or ignores the fact that there is a long history of the acceptance of this more reassuring view by scientific workers in the field, from the time of Charles Babbage to the present (*2*). Apparently Wiener shares some of the lack of understanding which he ascribes to the public, at least to the extent that he reads implications into some of the recent work which the workers themselves deny.

It is my conviction that machines cannot possess originality in the sense implied by Wiener and that they cannot transcend man's intelligence. I agree with Wiener in his thesis that "machines can and do transcend some of the limitations of their designers, and that in doing so they may be both effective and dangerous." The modern automobile travels faster than its designer can run, it is effective, and the records of highway fatalities attest to the dangerous consequences. However, a perusal of Wiener's article reveals that much more than this is meant, and it is to this extension of the thesis that I wish to take exception.

Wiener's reference to the "Sorcerer's Apprentice," and to the many tales based on the assumption that the agencies of magic are literal-minded, might almost lead one to think that he attributes magic to the machine. He most certainly seems to imply an equality between man and the machine when he states "disastrous results are to be expected not merely in the world of fairy tales but in the real world wherever two agencies essentially foreign to each other are coupled in the attempt to achieve a common purpose." In relationships between man and a machine the machine is an agency, but only an agency of man, entirely subservient to man and to his will. Of course, no one will deny that "we had better be quite sure that the purpose put into the machine is the purpose which we really desire and not merely a colorful imitation of it." If we want our house to be at 70°F when we get up in the morning, we had better set the thermostat at 70° and not at 32°. But once the thermostat is set at 70° we can go to sleep without fear that the genie in the furnace controls might, for some reason of his own, decide that 32° was a better figure. In exactly the same way and to the same degree we must anticipate our own inability to interfere when we instruct a modern digital computer (which works faster than we do) and when we instruct a thermostat (which works while we sleep).

Wiener's analogy between a machine and a human slave is also quite misleading. He is right in his assertion that "complete subservience and complete intelligence do not go together" in a human slave with human emotions and needs and with a will of his own. To ascribe human attributes to a machine simply because the machine can simulate some forms of human behavior is, obviously, a fallacious form of reasoning.

A machine is not a genie, it does not work by magic, it does not possess a will, and, Wiener to the contrary, nothing comes out which has not been put in, barring, of course, an infrequent case of malfunctioning. Programming techniques which we now employ to instruct the modern digital computer so as to make it into a learning machine *do not* "remove from the mind of the designer and operator an effective understanding of many of the stages by which the machine comes to its conclusions." Since the machine does not have a mind of its own, the "conclusions" are not "its." The so-called "conclusions" are only the logical consequences of the input program and input data, as revealed by the mechanistic functioning of an inanimate assemblage of mechanical and electrical parts. The "intentions" which the machine seems to manifest are the intentions of the human programmer, as specified in advance, or they are subsidiary intentions derived from these, following rules specified by the programmer. We can even anticipate higher levels of abstraction, just as Wiener does, in which the program will not only modify the subsidiary intentions but will also modify the rules which are used in their derivation, or in which it will modify the ways in which it modifies the rules, and so on, or even in which one machine will design and construct a second machine with enhanced capabilities. However, and this is important, the machine *will not* and *cannot* do any of these things until it has been instructed as to how to proceed. There is (and logically there must always remain) a complete hiatus between (i) any ultimate extension and elaboration in this process of carrying out man's wishes and (ii) the development within the machine of a will of its own. To believe otherwise is either to believe in magic or to believe that the existence of man's will is an illusion and that man's actions are as mechanical as the machine's. Perhaps Wiener's article and my rebuttal have both been mechanistically determined, but this I refuse to believe.

An apparent exception to these conclusions might be claimed for projected machines of the so-called "neural net" type. These machines were not mentioned by Wiener, and, unfortunately, they cannot be adequately discussed in the space available here. Briefly, however, one envisions a collection of simple devices which, individually, simulate the neurons of an animal's nervous system and which are interconnected by some random process simulating the organization of the nervous system. It is maintained by many serious workers that such nets can be made to exhibit purposeful activity by instruction and training with reward-and-punishment routines similar to those used with young animals. Since the internal connections would be unknown, the precise behavior of the nets would be unpredictable and, therefore, potentially dangerous. At the present time, the largest nets that can be constructed are nearer in size to the nervous system of a

flatworm than to the brain of man and so hardly constitute a threat. If practical machines of this type become a reality we will have to take a much closer look at their implications than either Wiener or I have been able to do.

One final matter requires some clarification—a matter having to do with Wiener's concluding remarks to the effect that "We must always exert the full strength of our imagination to examine where the full use of our new modalities may lead us." This certainly makes good sense if we assume that Wiener means for us to include the full use of our intelligence as well as of our imagination. However, coming as it did at the end of an article which raised the spectre of man's domination by a "learning machine," this statement casts an unwarranted shadow over the learning machine and, specifically, over the modern digital computer. I would be remiss were I to close without setting the record straight in this regard.

First a word about the capabilities of the digital computer. Although I have maintained that "nothing comes out that has not gone in," this does not mean that the output does not possess value over and beyond the value to us of the input data. The utility of the computer resides in the speed and accuracy with which the computer provides the desired transformations of the input data from a form which man may not be able to use directly to one which is of direct utility. In principle, a man with a pencil and a piece of paper could always arrive at the same result. In practice, it might take so long to perform the calculation that the answer would no longer be of value, and, indeed, the answer might never be obtained because of man's faculty for making mistakes. Because of the very large disparity in speeds (of the order of 100,000 to 1), on a computer we can complete calculations which are of immense economic value with great precision and with a reliability which inspires confidence, and all this in time intervals which conform to the demands of real-life situations. The magnitude of the tasks and the speed with which they are performed are truly breath-taking, and they do tend to impress the casual observer as being a form of magic, particularly when he is unacquainted with the many, many hours of human thought which have gone into both the design of the machine and, more particularly, into the writing of the program which specifies the machine's detailed behavior.

Most uses of the computer can be explained in terms of simulation. When one computes the breaking strength of an airplane wing under conditions of turbulence, one is, in effect, simulating the behavior of an actual airplane wing which is subjected to unusual stresses, all this without danger to a human pilot, and, indeed, without ever having to build the airplane in the first place. The checker-playing program on the I.B.M. 704, to which Wiener referred, actually simulates a human checker player, and the machine learns by accumulating data from its playing experience and by using some of the logical processes which might be employed by a person under similar circumstances. The specific logical processes used are, of course, those which were specified in advance by the human programmer. In these, and in many other situations, the great speed of the computer enables us to test the outcome resulting from a variety of choices of initial actions and so to choose the course with the highest payoff before the march of human events forces us to take some inadequately considered action. This ability to look into the future, as it were, by simulation on a computer is already being widely used, and as time goes on it is sure to find application in more and more aspects of our daily lives.

Finally, as to the portents for good or evil which are contained in the use of this truly remarkable machine—most, if not all, of man's inventions are instrumentalities which may be employed by both saints and sinners. One can make a case, as one of my associates has jokingly done, for the thesis that the typewriter is an invention of the devil, since its use in the nations' war offices has made wars more horrible, and because it has enslaved the flower of our young womanhood. On the whole, however, most of us concede that the typewriter, as a labor-saving device, has been a boon, not a curse. The digital computer is something more than merely another labor-saving device, since it augments man's brain rather than his brawn, and since it allows him to look into the future. If we believe, as most scientists do, that it is to our advantage to increase the rate at which we can acquire knowledge, then we can hardly do otherwise than to assert that the modern digital computer is a modality whose value is overwhelmingly on the side of the good. I rest my case with this assertion.

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31 May 1960

Spring Peak of Strontium-90 Fallout

Abstract. An increasing trend of Sr^{90} concentration in rain observed at Fayetteville, Ark., after November 1959, indicates that the seasonal and global movements of stratospheric air masses, such as described by Brewer (*1*) and Dobson (*2*), play an important role in causing the spring peaks of the Sr^{90} fallout.

To explain the vertical distributions of water vapor and ozone in the atmosphere, Brewer (*1*), in 1949, and Dobson (*2*), in 1956, proposed a model of global movement of the air masses. According to this model, there is a cold pool of air in the stratosphere over the winter pole during the late winter months, and it carries ozone-rich air to the lower levels in early spring. It was further suggested by Dobson that, if there is such a slow sinking of air in the middle latitudes from the stratosphere to the troposphere, it must be balanced elsewhere by a reverse current from the troposphere to the stratosphere.

Stewart *et al.* (*3*), in 1957, explained the spring peaks in the rate of stratospheric Sr^{90} fallout on the basis of the Brewer-Dobson model, and Burton *et al.* (*4*) reported that the Po^{210}/Pb^{210} ratios in the samples of air filter and rain can also be explained on the basis of the global circulation model of air masses. Kuroda (*5*) observed the 1958 spring peak of the stratospheric Sr^{90} fallout, and Fry *et al.* (*6*) pointed out the fact that the Sr^{90} concentrations in rain and snow remained fairly constant during the winter and spring months of 1958–59, despite the fact that this nuclide decays with a half-life of 51 days. They explained this as due to a marked increase in the rate of transfer of the fission products from the stratosphere to the troposphere in early spring months.

An alternate explanation for the spring peak of the rate of stratospheric fallout was proposed by Martell (*7*) in 1959, and this view was later sup-

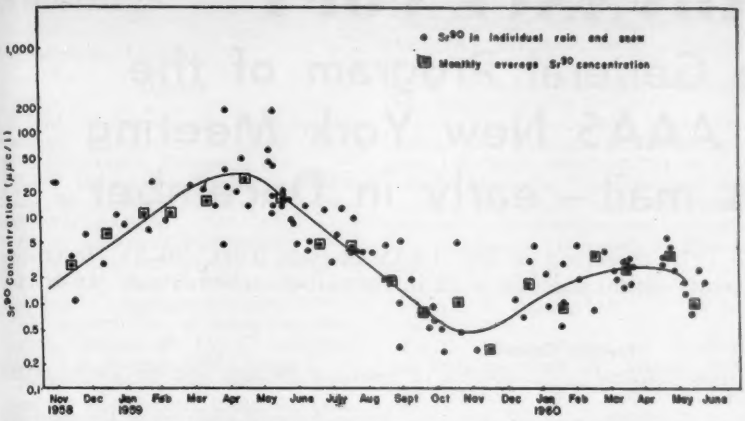

Fig. 1. Variation of the Sr^{90} concentration in rain and snow at Fayetteville, Ark.

ported by Libby (*8*). According to Martell, the clouds of stratospheric debris from middle-latitude and arctic U.S.S.R. detonations apparently reach the lower stratosphere after only limited diffusion and release substantially all of their contamination into a zone of restricted latitude during the first 6 months or so.

We have measured the Sr^{90} concentrations in individual samples of rain and snow collected at Fayetteville, Ark., during the period between November 1958 and May 1960, and the results are shown in Fig. 1. The monthly average Sr^{90} concentration in rain (C) was calculated from the equation.

$$\bar{C} = \Sigma F / \Sigma R,$$

where ΣF is the total amount of Sr^{90} (in micromicrocuries per square meter) transported by rain during the month period and ΣR is the total rainfall (in millimeters) during the same period. Our data show a marked seasonal variation of the Sr^{90} concentration in rain which follows a cyclic pattern with a maximum in the spring and a minimum in the fall, as would be expected from the global circulation model of air masses such as proposed by Brewer and Dobson. The cyclic pattern is similar to that of the seasonal variation of ozone in air reported by Paetzold (*9*).

The Sr^{89}/Sr^{90} ratios in the samples of rain and snow are plotted in Fig. 2, together with the data obtained by W. F. Libby at Washington, D.C., during the first 4 months of 1959. Fry (*10*) has recently estimated from the isotope ratio data that approximately 60 percent of the Sr^{90} fallout during the first half of 1959 originated from the fall 1958 U.S.S.R. test series. Figure 2 indicates that the fraction of the Sr^{90} which originated from the "older" debris has somewhat increased during the second half of 1959.

It is interesting to note that Feely (*11*) has recently reported that a mean stratospheric residence time of less than 18 months has to be assumed to explain the presence of only 1.0 megacurie of Sr^{90} in the stratosphere by mid-1958. Our data seem to be explained quite well by the assumption that whereas the transfer from the stratosphere occurred at a maximum rate in the spring months and at a minimum rate in the fall months, the over-all rate of the Sr^{90} transfer from the stratosphere during this period was roughly equivalent to an "apparent" stratospheric mean storage time of approximately 1 year or even less (*12*).

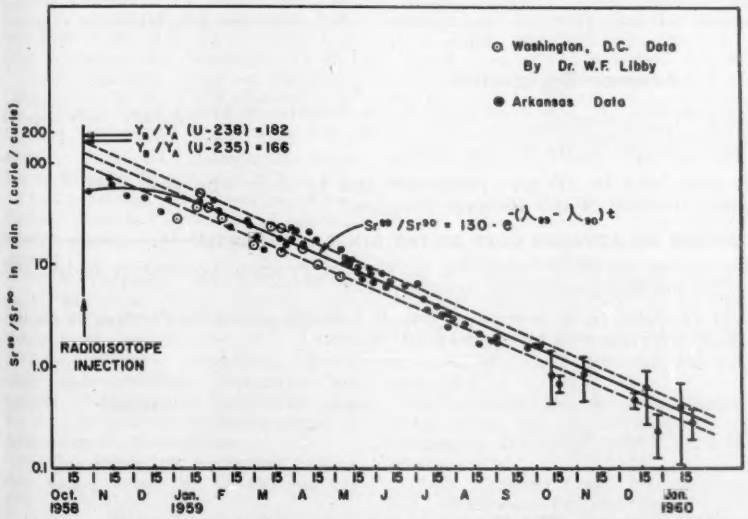

Fig. 2. Variation of the Sr^{89}/Sr^{90} ratio in rain and snow at Fayetteville, Ark.; y_B/y_A is the ratio of the fission products B and A (Sr^{89} and Sr^{90}) freshly produced by a nuclear explosion.

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12. This investigation was made possible by support from the U.S. Atomic Energy Commission. We are grateful to Miss Fay Ann Jew for assisting in analytical work, and to T. K. Chan, J. M. Bailey, and M. Dobbs for assisting in counting and sampling work.

7 June 1960

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14–15. Society of Photographic Scientists and Engineers, symp., Washington, D.C. (F. M. Brown, Photomechanisms, Inc., Box 67, Huntington Station, N.Y.)

15. American Soc. of Safety Engineers, annual, Chicago, Ill. (A. C. Blackman, ASSE, 5 N. Wabash Ave., Chicago 2)

15–16. American Acad. of Psychotherapists, 5th annual conf., Cleveland, Ohio. (B. J. Barkley, 1856 Coventry Rd., Cleveland Heights 18, Ohio)

16. American College of Dentists, Los Angeles, Calif. (O. W. Brandhorst, 4236 Lindell Blvd., St. Louis 8, Mo.)

16–22. High-Speed Photography, 5th intern. cong., Washington, D.C. (V. H. Allen, Soc. of Motion Picture and Television Engineers, 55 W. 42 St., New York 36)

16–22. Society of Motion Picture and Television Engineers, semi-annual conv., Washington, D.C. (C. S. Stodter, SMPTE, 55 W. 42 St., New York 36)

17–18. Basic Science in France and the United States, symp., New York, N. Y. (S. Roth, Office of Research Services, New York Univ., Washington Square Center, New York 3)

17–19. Adaptive Control Systems, symp., New York, N.Y. (H. Levenstein, W. L. Maxon Corp., 260 W. 34 St., New York)

17–19. American Oil Chemists' Soc., fall, New York, N.Y. (W. C. Ault, U.S. Dept. of Agriculture, Philadelphia 18, Pa.)

17–20. American Acad. of Pediatrics, Chicago, Ill. (E. H. Christopherson, 1801 Hinman Ave., Evanston, Ill.)

17–20. American Dental Assoc., Los Angeles, Calif. (H. Hillenbrand, ADA, 222 E. Superior St., Chicago, Ill.)

17–21. Neutron Pile Research, symp., Vienna, Austria. (International Atomic Energy Agency, 11 Kärntner Ring, Vienna 1)

17–22. Diagnosis and Treatment of Acute Radiation Injury, Geneva, Switzerland. (World Health Organization, Palais de Nations, Geneva)

17–26. Plastics Processing, intern. cong. and exhibition, Amsterdam and Utrecht, Netherlands. (Secretariat, c/o N. V. 't Raedthuys, Tesselschadestraat 5, Amsterdam-W, Netherlands)

18. Oak Ridge Inst. of Nuclear Studies, Oak Ridge, Tenn. (W. G. Pollard, Box 117, Oak Ridge)

18–20. Mathematical Optimization Techniques, symp., Berkeley, Calif. (R. M. Oliver, Dept. of Industrial Engineering, Univ. of California, Berkeley 4)

18–21. American Dietetic Assoc., 43rd annual, Cleveland, Ohio. (M. L. Ross, Simmons College, The Fenway, Boston 15, Mass.)

19–20. American Geophysical Union, Moscow, Idaho. (A. N. Sayre, U.S. Geological Survey, Washington 25, D.C.)

19–21. Design of Experiments, 6th conf. (by invitation only), Aberdeen Proving Ground, Md. (F. G. Dressel, Office of Ordnance Research, Box CM, Duke Station, Durham, N.C.)

19–21. Space Navigation, symp., Columbus, Ohio. (Institute of Radio Engineers, 1 E. 79 St., New York 21)

19–26. Measuring Techniques and Automation, 2nd intern. cong., Düsseldorf, Germany. (Nordwestdeutsche Ausstellungs-Gesellschaft, Ehrenhof 4, Düsseldorf)

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20–22. Institute of Management Sciences, 7th intern., New York, N.Y. (J. Townsend, IMS, 30 E. 42 St., New York 17)

21–22. Association of Midwestern College Biology Teachers, 4th annual, Mankato, Minn. (L. Zell, Mankato State College, Mankato, Minn.)

21–22. Research Approaches to Psychiatric Problems, symp., Galesburg, Ill. (T. T. Tourlentes, Galesburg State Research Hospital, Galesburg)

21-23. American College of Cardiology, St. Louis, Mo. (G. F. Greco, 114–08 Linden Blvd., Ozone Park 16, N.Y.)

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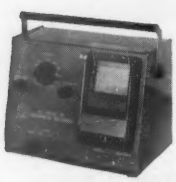

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November

1–3. International Cong. on Experimental Mechanics, New York, N.Y. (R. Guernsey, Jr., Soc. of Experimental Stress Analysis, General Engineering Lab., General Electric Co., Schenectady 5, N.Y.)

1–16. International Electrochemical Commission, New Delhi, India. (American Standards Assoc., 70 E. 45 St., New York 17)

2–4. Plasma Physics, 2nd annual, Gatlinburg, Tenn. (A. H. Snell, Oak Ridge Natl. Lab., Oak Ridge, Tenn.)

2–4. Society for Experimental Stress Analysis, Berkeley, Calif. (W. W. Murray, Massachusetts Inst. of Technology, Cambridge)

2–5. American Soc. of Parasitologists, Los Angeles, Calif. (F. J. Kruidenier, Zoology Dept., Univ. of Illinois, Urbana)

2–5. American Soc. of Tropical Medicine and Hygiene, Los Angeles, Calif. (R. B. Hill, 3573 St. Gaudens Rd., Miami 33, Fla.)

2-5. American Speech and Hearing Assoc., Los Angeles, Calif. (K. O. Johnson, 1001 Connecticut Ave., NW, Washington 6)

3–4. Electrostatic Propulsion, conf., Monterey, Calif. (J. M. Sellen, Thompson Ramo-Wooldridge, Inc., 8433 Fallbrook Ave., Canoga Park, Calif.)

3–4. Muscle as a Tissue, conf., Philadelphia, Pa. (Division of Research, Lankenau Hospital, Philadelphia 31)

4–5. West-Central States Biochemical Conf., Lincoln, Neb. (J. H. Pazur, Dept. of Biochemistry and Nutrition, Univ. of Nebraska, Lincoln)

4–6. Assoc. of Clinical Scientists, Washington, D.C. (R. P. MacFate, 54 W. Hubbard St., Chicago 10, Ill.)

5. Society for Industrial and Applied Mathematics, Philadelphia, Pa. (G. Kaskey, Remington Rand Univac, 1900 W. Allegheny Ave., Philadelphia)

7–10. Society of Exploration Geophysicists, 30th annual intern., Galveston, Tex. (C. C. Campbell, Box 1536, Tulsa 1, Okla.)

9–10. Use of Secondary Surfaces for Heat Transfer with Clean Gases, symp., London, England. (Secretary, Institution of Mechanical Engineers, 1 Birdcage Walk, London, S.W.1)

9–11. Clinical Chemistry Methods, symp., Cleveland, Ohio. (A. Hainline, Cleveland Clinic, 2020 E. 93 St., Cleveland 6)

10–12. Geological Soc. of America, 73rd conv., Denver, Colo. (H. R. Aldrich, GSA, 419 W. 117 St., New York 27)

10–12. National Assoc. of Geology Teachers, Denver, Colo. (F. Foote, Dept. of Geology, Williams College, Williamstown, Mass.)

10–13. Pacific Coast Fertility Soc., Las Vegas, Nev. (A. C. Wineberg, 3120 Webster St., Oakland, Calif.)

11–12. Paleontological Soc., Denver, Colo. (H. B. Whittington, Harvard Univ., Cambridge 38, Mass.)

13–16. Society of American Foresters, 60th annual, Washington, D.C. (H. Clapper, SAF, 825 Mills Bldg., Washington 6)

14–17. Magnetism and Magnetic Materials, 6th annual conf., New York, N.Y. (L. R. Bickford, Jr., I.B.M. Research Center, Yorktown Heights, N.Y.)

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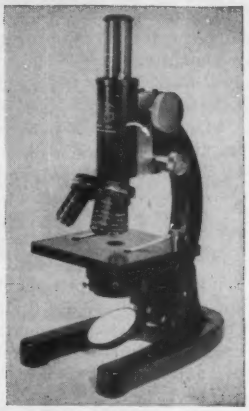

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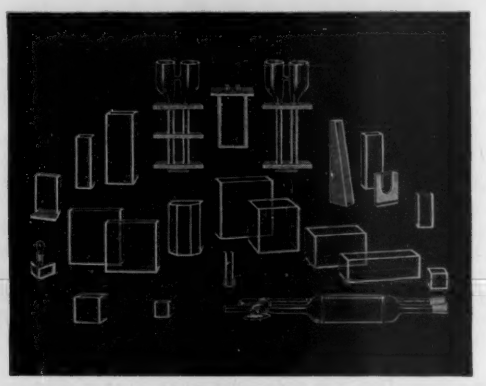

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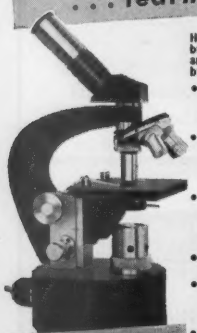

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■ RECORDER for Pirani-type vacuum gage is a strip-chart galvanometer type. Marking is rectilinear on pressure-sensitive paper; galvanometer is clamped for marking every 2 seconds. Gear trains are available for various synchronous speeds. Vacuum ranges are 0 to 25 and 0 to 1000 μ-Hg. Accuracy is said to be ± 2 percent of full scale. Dimensions are 3⅝ by 5⅝ by 4⅛ in. and weight with 63-ft chart roll is 3½ lb. (Arthur F. Smith, Inc., Dept. Sci729A, 311 Alexander St., Rochester 4, N.Y.)

■ COUNTER-TIMER is an all solid-state instrument that totalizes random events over any of eight time intervals from 10^{-6} to 10 seconds, and measures frequency from 1 to 10^6 cy/sec; period from 10^{-6} to 10^7 seconds, and phase angle from 0 to 180 deg. The crystal-controlled clock signal, said to be stable to $\pm 0.3 \times 10^{-6}$ sec/wk, is brought out to the front panel. Square waves in seven decade steps from 0.1 to 10^5 cy/sec are also provided. (Transistor Specialties, Inc., Dept. Sci730A, Terminal Drive, Plainview, N.Y.)

■ SURFACE INDICATOR ACCESSORY has a sapphire stylus with a chisel point 1/16 in. wide and a radius of 0.0005 in. Its use permits surface measurements of cutting edges, knife edges, and components with very small outside diameter (such as wire) in the range 0 to 1000 μin. The maximum force exerted by the stylus is 2 gm. (Brush Instruments Div. of Clevite Corp., Dept. Sci775, 37th and Perkins Ave., Cleveland 14, Ohio)

■ PRESSURE CALIBRATOR consists of a frequency-modulated oscillator, an integral power supply, and a plug-in unit containing a selected, precise, pressure pickup with heater and thermostat. Plug-in units are interchangeable, covering absolute and differential ranges from 10 to 3000 lb/in.^{2} and gage pressures up to 10,000 lb/in.^{2} Output can be displayed on a frequency counter or on printed paper tape. Output frequency is 10 kcy/sec at zero-input. Frequency deviation of 2.5 kcy/sec may be multiplied by 2 or 4 by a plug-in accessory. Combined linearity and hysteresis is ± 0.05 percent for ranges up to 2500 lb/in.^{2} and ± 0.08 percent for ranges above 2500 lb/in.^{2} Higher accuracy is available at increased cost. (Wiancko Engineering Co., Dept. Sci768, 255 N. Halsted Ave., Pasadena, Calif.)

■ IONIZATION VACUUM PUMP is said to achieve enhanced speed and stability by virtue of a three-electrode construction that includes a sputtering cathode. Speeds claimed are: air, 25 lit./sec.; argon, 8 lit./sec; methane, 28 lit./sec; and hydrogen, 15 lit./sec. Speed is constant over the operating pressure range of the pump. Ultimate pressure is less than 10^{-10} mm-Hg. The pump can be baked at 450°C. Life of the replaceable sputtering cathode is more than 10,000 hours at 10^{-6} mm-Hg. (Consolidated Vacuum Corp., Dept. Sci750, 1775 Mt. Read Blvd., Rochester 3, N.Y.)

■ PHOTOMICROGRAPHY ADAPTER permits a still or motion-picture camera to be used with microscopes. Exposures are made with the camera lens set at infinity. A viewing system with cross hairs permits the specimen to be viewed and the field of view selected and focused. All the light is used for the exposure and is deflected for viewing. The camera shutter is used in the normal fashion. (Lafayette Radio, Dept. Sci788, 165-08 Liberty Ave., Jamaica 33, N.Y.)

■ TIME CODE GENERATOR furnishes a 36-bit code indicating time of day and day of year in a proposed Inter-Range Instrumentation Group format and is read out once each second at a 100-pulse/sec rate. A leap-year switch provides for the extra day every 4 years. Two outputs are in d-c–level–shift form and modulated 1000 cy/sec carrier. Stability is said to be 3 parts in 10^9. Provision is made for synchronization with station WWV. (Electronic Engineering Co., Dept. Sci778, 1601 E. Chestnut Ave., Santa Ana, Calif.)

■ ELECTRONIC MICROMETER is an opposed-head device that permits measurement from two sides to the center line of a fragile or compressible part. Opposing micrometer tips may be zeroed against each other at a selected reference position or at the anvil surface. Controls bring the upper tip downward, to touch the top of the part being measured, and the lower tip upward through a hole in the lower anvil to contact the bottom. Measured values are displayed on counters to 20 μin. (J. W. Dice Co., Dept. Sci782, Englewood, N.J.)

■ SOUND-PRESSURE ALARM operates relay contacts at a preset sound or vibration level. The standard model operates at 70 to 100 db sound pressure level and has a differential of 2 db. An electronic delay prevents actuation on single impacts. (H. H. Scott, Inc., Dept. Sci777, 111 Powder Mill Rd., Maynard, Mass.)

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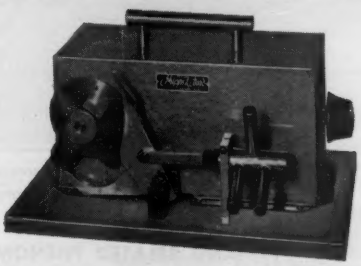

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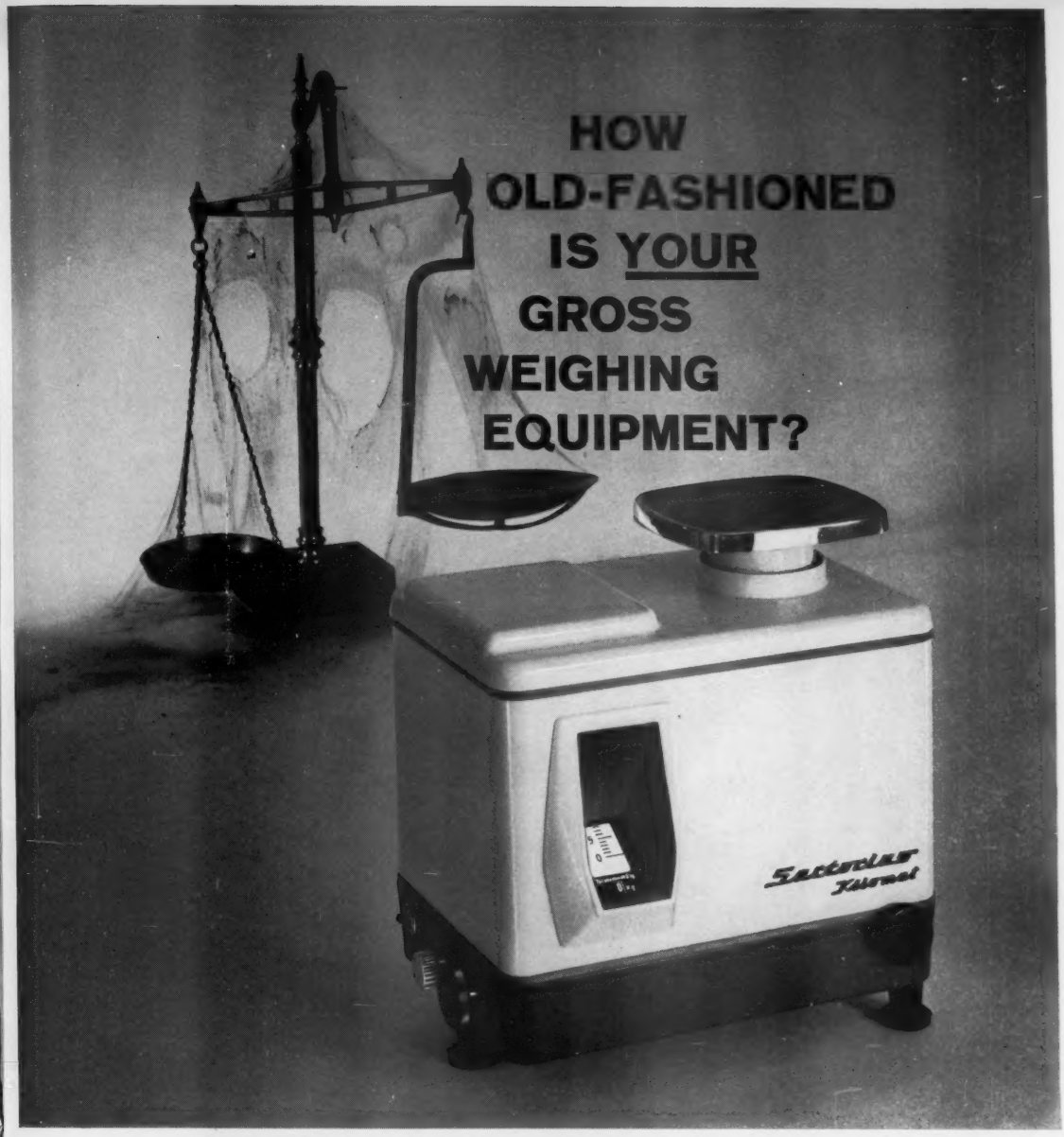

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Sample emission is determined by manually balancing its effect with that of a reference beam from the same light source upon a single photomultiplier tube, employing a rotating light interrupter to pass sample and reference beams alternately. Difference in relative a.c. detector output, amplified by a highly stable first stage, is sensed by a phase shift detector and fed to a null-type meter. Intensity of reference beam is regulated by a linear cam in the light path, which permits calibration in terms of 100 equal divisions on the "Fluorescence" dial. Design eliminates dark current effects; light source, detector and line voltage variations; and zero point error.

Takes 2-inch square filters for operation at 405, 436 or 546 mmu with 4-watt lamp and various secondary filters. Requires 2.5 to 4 ml samples in either standard test tubes, 10 to 12 mm inside diameter, or in 10 mm square cuvettes.

Adequate 3600A u.v. excitation is obtained with only 4-watt lamp output, thereby minimizing sample deterioration by light or heat. Interchangeable lamp (254 mmu), quartz cuvettes and special filters are available for 2537A operation.

Fluorometer, Turner, complete outfit with general purpose accessories for 3600A operation, consisting of set of five matched cuvettes 75 x 12 mm, primary filter 360 mmu, secondary filter 415 mmu, spare 4-watt ultraviolet lamp, and service manual; for 115 volts, 50 or 60 cycles, a.c. **985.00**

Square Cuvette, of Pyrex brand glass, precision bore, 75 x 10 mm; only accessory required for nephelometry **10.00**

Uranium Pellet Door, interchangeable with standard door. Standard fusion pellets drop into place and can be ejected by a spring device **45.00**

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